THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR APRIL 1855.

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TO READERS AND CORRESPONDENTS.

The following works have been received:—

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXVII. London, 1854. (From the Society.)

What to Observe at the Bedside and after Death in Medical Cases. Published under the authority of the London Medical Society of Observation. Second American from the second and enlarged London edition. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

The Principles and Practice of Obstetric Medicine and Surgery in reference to the Process of Parturition, with sixty-four plates and numerous wood-cuts. By Francis H. Ramsbotham, M. D., Fellow of the Royal College of Physicians of London, Obstetric Physician to the London Hospital, etc. etc. A new American edition, revised by the Author. With notes and additions, by William V. Keating, M. D., A. M., Lecturer on Obstetrics and Diseases of Women in the Philadelphia Medical Association, Physician to St. Joseph's Hospital, etc. etc. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

A Practical Treatise on the Diseases peculiar to Women. Illustrated by cases derived from Hospital and Private Practice. By Samuel Ashwell, M. D., M. R. C. of P. L., and late Obstetric Physician and Lecturer to Guy's Hospital. Third American from the third and revised London edition. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

A Treatise on the Practice of Medicine. By Geo. B. Wood, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania, President of the College of Physicians of Philadelphia, etc. etc. Fourth edition. Philadelphia: Lippincott, Grambo, & Co., 1855. (From the Author.)

Outlines of Chemical Analysis Prepared for the Chemical Laboratory at Giessen. By Dr. Heinrich Will, Professor of Experimental Chemistry in the University at Giessen. Translated from the third German edition, by Daniel Breed, of the U. S. Patent Office, etc., and Lewis H. Steiner, M. A., M. D., Professor of Chemistry in the National Medical College. Boston and Cambridge: James Munroe & Co., 1855. (From the Publishers.)

Autobiography of Charles Caldwell, M. D. With a Preface, Notes, and Appendix. By Harriot W. Warner. Philadelphia: Lippincott, Grambo, & Co., 1855. (From the Publishers.)

Illustrated Manual of Operative Surgery and Surgical Anatomy. By MM. Ch. Bernard, D. M. P., and Ch. Huette. Edited, with notes and additions, and adapted to the use of the American Medical Student, by W. H. Van Buren, M. D., Professor of Anatomy in the University Medical College, etc., and C. E. Isaacs, M. D., Demonstrator of Anatomy in the University Medical College, etc. Illustrated with steel engravings, from drawings after nature, by M. J. Leveille. Designed to serve as a companion to the ordinary Text-books of Surgery. New York and London: H. Bailliere. Paris: J. B. Bailliere, 1855. (From the Publishers.)

The Medicinal, Poisonous, and Dietetic Properties of the Cryptogamic Plants of the United States. Being a report made to the American Medical Association at its Sessions, held in Richmond, Va., and St. Louis, Mo. By Francis Peyre Porcher, M. D., etc. From the Transactions of the American Medical Association, Vol. VII. (From the Author.)

Report on the Epidemics of Ohio, Indiana, and Michigan, for the years 1852 and 1853. By Geo. Mendenhall, M. D. (From the Author.)

Medicine a Scheme or Disease a Unit. By H. Backus. Selma, Ala., 1855. (From the Author.)

Puerperal Fever as a Private Pestilence. By Oliver Wendell Holmes, M. D., Parkman Professor of Anatomy and Physiology in Harvard University. Boston: Ticknor & Fields, 1855.

Report of the Board of Health of the City of Washington for the year ending June 30, 1854.

Exsection of the Head of the Femur and Removal of the Upper Rim of the Acetabulum for Morbus Coxarius. By Lewis A. Sayre, Surgeon to Bellevue Hospital, etc. etc. (From the New York Journal of Medicine.) New York, 1854. (From the Author.)

History and Observations on Asiatic Cholera in Brooklyn, N. Y., in 1854. By Joseph C. Hutchison, M. D., late Physician to the Brooklyn Cholera Hospital, etc. (From the New York Journal of Medicine.) New York, 1855. (From the Author.)

Report of the Board of Health of the City of Brooklyn to the Common Council, December, 1854. Brooklyn, 1854. (From Joseph C. Hutchison, M. D.)

Second Annual Report of the Surgeons of the New York Ophthalmic Hospital for the year 1854. New York, 1855.

Report of the Inspectors of the Western Penitentiary of Pennsylvania for 1854. Pittsburg, 1855. (From Dr. Pollock.)

Physician's Report on closing the Roper Hospital after the Yellow Fever Epidemic of 1854. By Wm. S. Wragg, M. D., Physician to the Hospital. Charleston, 1854.

Sanitary Reports of the City of Buffalo for the year 1854. Printed by order of the Common Council. Buffalo, 1855.

Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of 1853. Published by authority of the City Councils of New Orleans. New Orleans, 1854. (From Dr. Barton.)

Lectures in reply to the Croonian Lectures for 1854, of Charles West, of London, on the Pathological Importance of Ulceration of the Os Uteri. By Henry Miller, M. D., Professor of Obstetric Medicine in the University of Louisville, etc. Extracted from the Western Journal of Medicine and Surgery. Louisville, 1855.

The Relative Merits of Empiricism and Rationalism in the present state of Medical Science. By Alexander Henry, M. D. Read before the Medical Society of London, March 25, 1854. Reprinted from the Association Medical Journal, 1854. (From the Author.)

A Brief Sketch of the History of Lexington, Ky., and of Transylvania University. Delivered as an Introductory Lecture to the Winter Course in the Medical Department of Transylvania University. By Robert Peter, M. D., Professor of Chemistry. Lexington, 1854.

Address delivered before the Philadelphia County Medical Society. By Thos. F. Betton, M. D., M. A. N. S., etc., December 13, 1854. Published by the Society. Philadelphia, 1854.

Anniversary Discourse before the New York Academy of Medicine. Delivered in Clinton Hall, November 22, 1854. By John H. Griscom, M. D. (Published by the Academy.) New York, 1855. (From the Author.)

Memoir of John A. Swett, M. D., late Professor of the Theory and Practice of Medicine in the Medical Department of the University of the City of New York. By B. W. M'Cready, M. D. Read before the New York Medical and Surgical Society, and published by its order. New York, 1855. (From Dr. Du Bois.)

Address to the Graduates of the Medical Department of Pennsylvania College. By J. M. Allen, M. D., Professor of Special and Surgical Anatomy. With a list of Graduates. Philadelphia, 1855.

Oration delivered before the Physico-Medical Society of New Orleans, at their Anniversary Meeting, held December, 1854. By A. Mercier, M. D., P., Principal Surgeon to the Circus Street Hospital. New Orleans, 1854. (From the Author.)

Annual Reports of the Commissioners and Superintendents of the Indiana Hospital for the Insane, to the General Assembly. November, 1854. Indianapolis, 1855.

Twelfth Annual Report of the Managers of the State Lunatic Asylum. Transmitted to the Legislature, January 17, 1855. Albany, 1855. (From Geo. W. Bradford, M. D.)

Report of the Pennsylvania Hospital for the Insane for the year 1854. By THOMAS S. KIRKBRIDE, M. D., Physician to the Institution. Published by order of the Board of Managers. Philadelphia, 1855. (From the Author.)

Report of the Board of Managers of the State Lunatic Asylum to the Eighteenth General Assembly. Jefferson City, 1855.

Nineteenth Annual Report of the Managers of the New York Institution for the Blind to the Legislature of the State. Made in conformity to law. January, 1855, for the preceding year. New York, 1855.

Fourth Biennial Report of the Trustees of the Illinois State Hospital for the Insane, December, 1854. Jacksonville, 1854. (From Dr. Frye.)

Annual Report of the Trustees and Superintendent of the State Lunatic Asylum of Pennsylvania, located at Harrisburg, for 1854. Harrisburg, 1855. (From Dr. Curwen.)

Second Annual Report to the General Assembly of Kentucky, relating to the Registry and Returns of Births, Marriages, and Deaths. From Jan. 1, 1853, to Dec. 31, 1853. Frankfort, 1854. (From Dr. Sutton.)

The Transactions of the New Hampshire Medical Society. (Sixty-fourth Anniversary.) Held at Concord, June 6 and 7, 1854. Concord, 1854. From Dr. Hubbard.)

The following Journals have been received in Exchange:—

Archives d'Ophthalmologie. Par M. A. JAMAIN, Docteur en Médecine de la Faculté de Paris. Sept., Oct. 1854.

Le Moniteur des Hôpitaux. Dec. 1854, Jan., Feb. 1855.

Revue de Thérapeutique Médico-Chirurgicale. Par A. MARTIN-LAUZER. Dec. 1854.

Gazette Médicale de Paris. Dec. 1854, Jan., Feb. 1855.

Annales de la Sociedad de Médecina, Montevideana, Nov. 1853, Jan., June, July, 1854.

The Glasgow Medical Journal. Oct. 1854, Jan. 1855.

Medical Times and Gazette. Dec. 1854, Jan., Feb. 1855.

Dublin Medical Press. Dec. 1854, Jan., Feb. 1855.

The Dublin Hospital Gazette. Dec. 1854, Jan., Feb. 1855.

The Edinburgh Medical and Surgical Journal. April, July, Oct. 1854, Jan. 1855

The Journal of Psychological Medicine and Mental Pathology. Edited by FORBES WINSLOW, M. D. Jan. 1855.

Association Medical Journal. Edited by JOHN ROSE CORMACK, M. D. Aug., Sept., Oct., Nov., Dec. 1854, Jan. 1855.

The Retrospect of Medicine. Edited by W. BRAITHWAITE. July, Dec. 1854.

Monthly Journal of Medicine. Dec. 1854, Jan., Feb. 1855.

The British and Foreign Medico-Chirurgical Review. Jan. 1855.

The Half-Yearly Abstract of the Medical Sciences. Edited by W. H. RANKING, M. D., and C. B. RADCLIFFE, M. D. July—Dec. 1854.

The Dublin Quarterly Journal of Medical Science. Feb. 1855.

The Medical Chronicle or Montreal Monthly Journal. Edited by W. WRIGHT, M. D., and D. C. MACCALLUM, M. D. Jan., Feb., March, 1855.

The Boston Medical and Surgical Journal. Edited by J. V. C. SMITH, M. D., and GEO. S. JONES, M. D. Jan., Feb., March, 1855.

The Peninsular Journal of Medicine. Edited by A. B. PALMER, M. D., and E. ANDREWS, M. D. Jan., Feb., March, 1855.

Southern Medical and Surgical Journal. Edited by L. A. DUGAS, M. D. Jan., March, 1855.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D. Jan., March, 1855.

Nashville Journal of Medicine and Surgery. Edited by W. K. BOWLING, M. D., and PAUL F. EVE, M. D. Jan., March, 1855.

The Medical Counsellor. Edited by R. HILLS, M. D. Jan., Feb., March, 1855.

New York Dental Recorder. Edited by CHARLES W. BALLARD, D. D. S. Jan., Feb. 1855.

Charleston Medical Journal and Review. Edited by D. J. Cain, M. D., and F. Peyre Porcher, M. D. Jan., March, 1855.

The American Journal of Dental Science. Edited by Chapin A. Harris, M. D., D. D. S., and A. Snowden Piggot, M. D. Jan. 1855.

The New Orleans Medical and Surgical Journal. Edited by Bennet Dowler, M. D. Jan. 1855.

The New Hampshire Journal of Medicine. Edited by Geo. H. Hubbard, M. D. Jan., Feb., March, 1855.

The Stethoscope. Edited by Drs. Atkinson, Haxall, Bolton, Lewis, Merritt, and Wilson. Jan., Feb., March, 1855.

The American Medical Monthly. Edited by E. H. Parker, M. D. Dec., 1854, Jan., Feb., March, 1855.

The American Journal of Science and Arts. Edited by Professors B. Silliman, B. Silliman, Jr., and J. D. Dana, in connection with Professors Gray and Agassiz, and Dr. Gibbs. Jan., March, 1855.

Iowa Medical Journal. Conducted by the Faculty of the Medical Department of the Iowa University. Dec. and Jan. 1854–5.

American Journal of Pharmacy. Edited by Wm. Proctor, Jr. Jan., March, 1855.

The Medical Examiner. Edited by Samuel L. Hollingsworth, M. D. Jan., Feb., March, 1855.

The American Journal of Insanity. Edited by the Officers of the New York State Lunatic Asylum. Jan. 1855.

The Western Lancet. Edited by T. Wood, M. D. Nov. 1854, Jan., Feb., March, 1855.

The American Medical Gazette. Edited by D. Meredith Reese, M. D. Jan., Feb., March, 1855.

New York Medical Times. Edited by H. D. Bulkley, M. D., and J. G. Adams, M. D. Jan., Feb., March, 1855.

The Western Journal of Medicine and Surgery. Edited by Lunsford P. Yandell, M. D. Dec. 1854, Jan., Feb. 1855.

Memphis Medical Recorder. Edited by A. P. Merrill, M. D., and C. T. Quintard, M. D. Jan. 1855.

The New York Journal of Medicine. Edited by Samuel S. Purple, M. D., and Stephen Smith, M. D. Jan., March, 1855.

St. Louis Medical and Surgical Journal. Edited by Drs. Linton, McPheeters, Moore, and Deming. Nov. 1854, Jan., March, 1855.

The North-Western Medical and Surgical Journal. Edited by N. S. Davis, M. D., and H. A. Johnson, M. D. Dec. 1854, Jan., Feb., March, 1855.

The New Jersey Medical Reporter. Edited by S. W. Butler, M. D. Jan., Feb., March, 1855.

The Virginia Medical and Surgical Journal. Edited by Drs. J. B. McCaw, J. F. Peebles, and G. A. Otis. Jan., Feb., March, 1855.

New Orleans Medical News and Hospital Gazette. Edited by Drs. Choppin, Beard, Schlater, and Vance. Jan., Feb., March, 1855.

Buffalo Medical Journal. Edited by Austin Flint, M. D., and Sanford B. Hunt, M. D. Jan., March, 1855.

Communications intended for publication, and Books for Review, should be sent, *free of expense*, directed to Isaac Hays, M. D., Editor of the American Journal of the Medical Sciences, care of Messrs. Blanchard & Lea, Philadelphia. Parcels directed as above, and (carriage paid) under cover, to John Miller, Henrietta Street, Covent Garden, *London;* or M. Hector Bossange, Lib. quai Voltaire, No. 11, *Paris*, will reach us safely and without delay. We particularly request the attention of our foreign correspondents to the above, as we are often subjected to unnecessary expense for postage and carriage.

All remittances of money, and letters on the *business* of the Journal, should be addressed *exclusively* to the publishers, Messrs. Blanchard & Lea.

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Report of the Secretary of State of Connecticut, relating to the Registration of the Births, Marriages, and Deaths in Connecticut for the year ending August 7, 1848. 8vo. pp. 16.

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First Annual Report to the General Assembly of Kentucky, relating to the Registry and Returns of Births, Marriages, and Deaths, from Jan. 1 to Dec. 31, 1852 - - - - - - - - - - - - 407

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X. Transactions of State Societies.

1. Transactions of the Tennessee State Medical Society, at its Twenty-Fourth Annual Session, convened at Nashville May 4, 1853. 8vo. pp. 79.
2. The Transactions of the Iowa State Medical and Chirurgical Society, Third and Fourth Sessions, held at Fairfield May, 1852, and Davenport June, 1853. 8vo. pp. 48.
3. Transactions of the Medical Society of the State of New York, at its Annual Meeting, in the City of Albany, held February, 1854. 8vo. pp. 148.
4. The Minutes of the Fifth Annual Meeting of the Medical Society of the State of Georgia, held in the City of Macon April 12, 1854. 8vo. pp. 55.
5. The Transactions of the New Hampshire Medical Society, Sixty-fourth Anniversary, held at Concord June 6 and 7, 1854. 8vo. pp. 56.
6. Proceedings of the American Pharmaceutical Association, at the Third Annual Meeting, held in Cincinnati July 25 and 26, 1854. 8vo. pp. 40. - - - - - - - - - - - - - - - 431

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XIII. Puerperal Fever, as a Private Pestilence. By Oliver Wendell Holmes, M. D., Parkman Professor of Anatomy and Physiology in Harvard University. 8vo. pp. 60. Boston, 1855.

On the Induction of Puerperal Fever, by Inoculation, so called. By Z. Pitcher, M. D. Published at the request of the Detroit Medical Society. Peninsular Journal of Medicine for February, 1855. - - - - 459

XIV. A Treatise on the Practice of Medicine. By George B. Wood, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia; One of the Physicians of the Pennsylvania Hospital; One of the Authors of the Dispensatory of the United States of America, etc. etc. Fourth Edition. 2 vols. 8vo. pp. 852, 864. Philadelphia, 1855. - - - 464

XV. The Principles and Practice of Obstetric Medicine and Surgery in Reference to the Process of Parturition. With sixty-four plates, and numerous wood-cuts. By Francis H. Ramsbotham, M. D., Fellow of the Royal College of London; Obstetric Physician to the London Hospital, and Lecturer on Obstetric and Forensic Medicine at the London Hospital

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Medical College, etc. etc. A new American Edition, revised by the Author. With Notes and Additions, by William V. Keating, M. D., A. M., Lecturer on Obstetrics and Diseases of Women in the Philadelphia Medical Association, Physician to the St. Joseph's Hospital, Member of the American Medical Association, Fellow of the College of Physicians, Member of the American Philosophical Society, etc. etc. Τὰ μὲν ἄλλα καὶ πόλεμος καὶ μεταβολὴ τύχης ἀνάλωσαν. ἡ Τέχνη δὲ σώζεταί. 8vo. pp. 648. Philadelphia, 1855, Blanchard & Lea. - - - - - - - - - 465
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THE

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FOR APRIL 1855.

ART. I.—*Observations upon the Kidney and its Excretions in different Animals.* By JOS. JONES, Student of Medicine in the University of Pennsylvania. (With 20 wood-cuts.)

THE object of this paper is to present a general view of the kidney and its excretions, and to direct attention to certain facts and principles which have been much overlooked. For the thorough investigation of this subject many years of hard labour will be required; since not only must correct quantitative analyses of the urine be made, but also of the blood of different animals. Our knowledge of the blood and of the secretions and excretions of animals, with the exception of those of the mammalia, is as yet very limited and indefinite; indeed, we know nothing whatever about the chemical and physical properties of the blood and urine of the cold-blooded animals.

I. *Comparative Anatomy of the Kidney in Vertebrate Animals.*

1. MAMMALIA.—The urinary apparatus in mammalia consists, with a few exceptions, of four distinct portions:—

1. The glands which separate the urine from the blood.
2. A duct which conveys this excretion to a reservoir.
3. A receptacle for the urine.
4. A canal leading from the reservoir to the exterior of the body, through which the urine is discharged out of the animal economy.

These parts exist in all mammalia except the monotremata, in which the fourth division of the apparatus is absent, and the ureters empty into the cloaca. The separation in these animals of the uro-urethral from the semino-urethral passages demonstrates conclusively that the existence of the penis is essentially and subordinately related to the sexual organs, and not to the renal.

The kidneys are situated, in all cases, in contact with the posterior walls of the abdominal cavity, on either side of the vertebral column and aorta, resting partly upon the psoas magnus muscle. The right kidney, owing to the position of the liver, is generally situated lower than the left. The exterior border of the kidney is convex, and looks outwards towards the parietes of the abdomen. The interior border looks towards the vertebral column, and is concave, being excavated by a deep fissure, in which enter the nerves, arteries, veins, and excretory ducts. When a section is made, it is found to consist of two distinct portions—an external, vascular (cortical), and an internal or tubular (medullary) substance.

The substance of the kidney is divided into pyramidal masses, having their bases towards the surface, whilst their apices extend into the pelvis or the branches of the ureter. The number of these pyramids varies in different animals. In many of the quadrumana, cheiroptera, insectivora, and rodentia, there is only a single pyramid, whilst in the seals there are one hundred, and in the dolphins two hundred. These characters are possessed in general by the kidneys of all vertebrata.

The exterior form of the organ differs in the various orders. In man, its shape is oval, resembling a kidney-bean, whence its name. An external form similar to this is found in the kidneys of the apes, cheiroptera, and many of the rodentia and ruminantia. In most carnivora they are much rounded. In the mink they are much elongated.

In several animals which have only a single pyramid of Malpighi, the apex is prolonged, gradually coming to a point, thus forming a vermiform appendage, which projects through the pelvis of the kidney into the ureter. This structure may be seen in the kidneys of the red bat (*Nycticeus Noveboracensis*), common mole (*Scálops Aquaticus*), and the house mouse (*Mus Musculus*).

In many cats and weasels, the kidneys are, upon their external surface, incompletely lobed, resembling those of new-born infants. Many animals, especially those inhabiting the water, have these organs divided externally into many distinct lobes. Of this form, the kidney of the common cow presents the most familiar example. This is elongated, and has from eighteen to twenty-four distinctly-marked lobes; each of these is in reality a separate kidney, having a cortical and medullary substance, forming a pyramid of Malpighi, the apex of which is surrounded by a branch of the ureter.

The enlarged dilated superior portion of the ureter, called the pelvis, does not exist in the cow; the ureter continues of a uniform size during the whole course, from its junction with the bladder to the point where it sends off branches to the different lobes. This peculiar lobulation of the kidney is well marked in other animals. In the otter there are twelve, and in the bear from forty to fifty lobes. The seals have this organ divided into from seventy to one hundred lobes; in consequence of this minute division, its surface presents a tessellated appearance. The kidneys of the true cetacea have a racemose appearance, and in those of the dolphin there usually exist two hundred lobes. The

external characters of the kidneys of these last-mentioned animals resemble those of some birds, as the turkey-buzzard (*Cathartes Aura*), and of all the reptiles, and several fishes, as the catfish (*Galeicthys Marinus*), the toadfish (*Batrachus Tau*), and the stingray (*Trygon Sabina*).

Above each kidney is situated a small body, called the supra-renal capsule, the office of which is as yet unknown. The form of this varies with different animals: thus, it is of the shape of a cocked hat in man, and the general shape of the kidney in the mole; and lobulated in the cetacea and other aquatic mammalia. This would seem to indicate that some relation exists between these two organs. In what that relation consists, we are at present unable to say, because we are ignorant of the office which the supra-renal capsules perform in the animal economy.

a. Minute Anatomical Structure of the Kidney in Mammalia.—The minute anatomical structure of these organs, in all the mammalia thus far examined, resembles in every respect that of the human kidney. We have examined carefully, under the microscope, the kidneys of various animals, as the cow, raccoon, rabbit, squirrel, and others, both injected and uninjected, and in every case the structure was the same. The tubuli uriniferi were seen, consisting of a basement membrane, lined with epithelial cells, and ending in club-shaped extremities and dilatations, which embraced the convoluted mass of a vein and artery, thus forming the corpuscles of Malpighi.

The microscopical anatomy of the kidney of man is described in every recent work on anatomy, and it is therefore unnecessary to do so here.

b. Development of the Tubuli Uriniferi.—The kidneys of a small fœtus of the common mouse, when examined under the microscope, were seen to consist, in large measure, of innumerable, elongated, and spheroidal cells. Amongst these, only a few short portions of the tubuli uriniferi were seen, and appeared to have been formed by the coalescence of these elongated cells. The kidneys of a fœtus of the same animal, in a more advanced state of development, was also examined. The ureter was distinctly seen entering for a short distance the substance of the kidney, connected with which were many short tubuli uriniferi. The ends of these short tubes were, in many instances, connected with chains of elongated cells, which had coalesced together. Some of the urinary tubes terminated in closed, rounded extremities, resembling the free extremities of cells which had not as yet united with others. The substance of the kidneys was composed, in great measure, of spheroidal and elongated cells, amidst which short portions of the urinary tubes were seen, having continuations and branches formed by the union of these elongated cells. These facts indicate that the urinary tubes are formed from spheroidal cells, which, elongating in opposite directions, coalesce, and the walls, where they come in contact, are removed by absorption.

c. Relative Size of the Kidneys in the different Orders of Mammalia.—Although our investigations of this subject are by no means complete, still,

we hope that the following facts and conclusions will interest and excite careful observers to investigate this subject for themselves. The relation existing between the weight of the kidneys and the weight of the body of an animal was obtained by accurately ascertaining the weight of the animal, and then of its kidneys, and dividing the former by the latter. The average weight of the bodies of several raccoons was found to be eighty times heavier than the average weight of their kidneys.

Compare this result, obtained from carnivorous animals, with the relative weights of the kidneys and bodies of animals which subsist entirely upon vegetable food. The rabbit was found to weigh one hundred and fifty-eight, and the gray squirrel one hundred and ninety times as much as their kidneys. The kidneys of the raccoon, a carnivorous animal, are relatively twice as large as those of the rabbit and squirrel, herbivorous animals.

If the character of the food, the structure and size of the digestive apparatus, and the habits of the two classes of animals be attentively considered, we will understand at once why this relative difference in size should exist in organs fulfilling the same office in both. The food of the carnivora, as the name implies, consists of flesh and blood, which is capable of ready digestion and absorption, and rapidly supplies the wants of the animal economy. The intestinal canal is in all cases short, when compared with that of the herbivora, and in some individuals is not much longer than the body.

Carnivorous animals have voracious, and, in some cases, almost insatiable appetites. They gorge themselves with food, which is capable of entering the circulation with little or no alteration, and rapidly supplying the wants of the economy. There must be some organ to act as a safety-valve, and remove rapidly the large, unnecessary quantities of nutriment which are so often received into the circulatory system. To accomplish this effectually, there must be correspondingly large organs.

In the herbivora, on the other hand, the intestinal canal is very long; in the case of the sheep, it is twenty-eight times the length of its body. In this class of animals, the food consists of vegetables, which require minute subdivision, and the principles which they contain must go through many metamorphoses before they are ready to supply the wastes and wants of the system. Consequently their introduction into the circulation is gradual, the elimination of the products of the disintegrations of the tissues, whose place they supply, must be correspondingly slow and gradual, and a much smaller-sized organ will perform the same office in them than in the carnivora.

2. Aves.—The kidneys of birds are impacted within deep cavities of the sacrum, and extend from the lungs to within a short distance of the anus. They retain the impressions of the last ribs, and the transverse processes of the sacrum. They are, in all cases, lobulated; each kidney having from three to five principal lobes. In several orders, their general form is determined

secondarily by that of the sacrum, and primarily by the habits of the birds. Thus, in cranes having a long slender body, they frequently come in contact in the median line, and even coalesce, as in the *Ardea Candidissima*.

In the chimney-swallow (*Chætura Pelasgica*) and in the woodpeckers, where the sacrum is broad, for the attachment of the strong muscles of the tail and legs, the kidneys are correspondingly broad and short.

In birds of prey, as the fish-hawk (*Pandion Haliætus*), which have a broad, deep sacrum for the support of the powerful muscles of the lower extremities, the kidneys are thick, broad, short, and deeply impacted within the cavities of the sacral bone. As far as our observations have extended, we may say that, as a general rule, the more powerful the exertions of a bird, the more active and energetic its habits of life, the more carefully will its kidneys be stowed away in the cavities of the sacrum. The necessity for this provision is evident when we consider the delicate nature of these organs, and the serious disturbance of the animal economy which immediately follows the infliction upon them of direct or indirect injuries. In the three other classes of vertebrate animals, the great Architect has placed them in contact with the posterior walls of the abdominal cavity, a position least of all liable to injury. In mammalia, reptiles, and fishes, the kidneys are protected by the vertebral column, and large and powerful dorsal muscles. In birds, these muscles are absent, because they are unnecessary in their peculiar structure and modes of locomotion. What better arrangement for the protection of these organs could have been devised, than that they should have been covered with bone at the part where they are most exposed? The peculiar form of the sacrum in birds, appears then to have been intended for the reception and protection of the renal organs.

Fig. 1 represents the kidneys of the small white crane (*Ardea Candidissima*), reduced to one-fourth its natural size, showing their union and coalescence in the median line. Fig. 2 represents the kidneys of the wood ibis (*Tantalus Loculator*), also reduced to one-fourth its natural size. The kidney of this bird presented, exteriorly, numberless small lobules, which are represented in the shaded portion of the drawing. These figures will give a better idea of the shape and positions of the renal organs, testicles, and supra-renal capsules, than a long and elaborate description.

The supra-renal capsules are small, flat, orange-colored bodies, always found on the mesial line of the anterior extremities of the kidneys. They are in contact externally with the large vascular trunks, and are generally covered by the testicles in the male, and the ovaries in the female. These organs are represented in Fig. 2 (*b*), and are seen lying above the testicles (*c*), which were remarkably small in this bird, in other respects an unusually large and fine specimen. In Fig. 1, we see that the supra-renal capsules are completely covered by the testicles (*a*, *a*).

The kidneys of struthious birds have small pyramids of Malpighi surrounded by calyces, and in the ostrich is found a true renal pelvis.

The ureters descend along the anterior surface of the kidneys, and terminate by perforating the cloaca posteriorly and superiorly. A true urinary

Fig. 1.

Fig. 2.

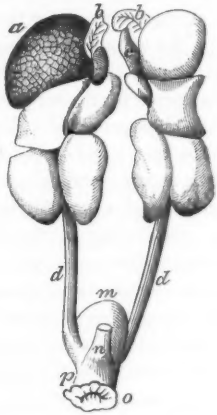


Fig. 1. Kidneys and testicles of the small white crane (*Aredea Candidissima*), one-fourth the natural size. *a, a.* Testicles. The supra-renal capsules lie between these and the anterior surface of the kidneys. *b.* Right kidney. *c.* Left kidney. These coalesce at their inferior portion. *d, d.* Spermatic cords. *n, m.* Ureters.

Fig. 2. Kidneys of the wood ibis (*Tantalus Loculator*), one-fourth the natural size. *a.* Superior lobe of right kidney, upon the surface of which are represented the small lobuli. *b, b.* Supra-renal capsules. *c.* Testicle. *d, d.* Ureters. *m.* Bladder-like expansion of the cloaca, considered by many anatomists a rudimentary bladder. *n.* Inferior portion of the rectum. *o.* Anus. *p.* Cloaca.

bladder is absent, although in many birds the ureters open into a distinct pouch-like dilatation of the cloaca. This may be seen in Fig. 2 (*m*). It is bounded above and below by a valvular fold, which separates it from the urethro-sexual cavity and the rectum (*n*).

a. Minute Anatomical Structure.—The kidneys of birds are composed of minute lobuli, which, in some individuals, as the *Tantalus Loculator*, can be distinguished externally. (Fig. 2, *a.*) In the turkey-buzzard (*Cathártes Aura*) they are convoluted, giving to the surface a tessellated aspect. In other birds, they are polyhedral. These lobules are connected with separate branches of the ureter, and have the shape of the pyramids of Malpighi. Towards their apices, they consist of converging tubuli uriniferi forming the medullary portion, whilst the cortical or outer parts consist of the branches and convolutions of these urinary tubes. These tubuli uriniferi, in all the birds whose kidneys we have examined under the microscope, end, as in mammalia in the Malpighian bodies, in loops, or in simple club-shaped extremities.

The absence of a pelvis and calices in the kidneys of most birds, appears to be absolutely necessary, when we consider the character of their urine,

which is semi-solid, consisting of innumerable small globules of the urate of ammonia. If there were any cavities in which this could accumulate, renal calculi would be of frequent occurrence. The existence of cilia in the cells lining the interior of the urinary tubes, has not as yet been accurately determined. Gerlach thinks that he has seen them on one occasion in the fowl. Although they were not noticed by us in our microscopical examinations of the kidneys of the *Pandion Haliætus*, *Ardea Discors*, and other birds, still, we think that it is highly probable that they do exist, because they would aid materially in the movement and expulsion of these minute globules of the urate of ammonia.

b. Relative Size of the Kidneys of Birds.—So far as our observations have extended, the same law holds good with birds as with mammalia. The kidneys are relatively larger in the carnivorous than in the frugivorous or granivorous birds. A glance at the following weights will render this evident:—

Weight of *Ardea Candidissima*, 86 times the weight of its kidneys.
" " *Discors*, 100 " " " "
" *Garrulus Cristatus*, 115 " " " "
" *Picus Auratus*, 121 " " " "

The first two birds are carnivorous, feeding upon fish and crustacea, whilst the last two live upon a mixed diet of berries and insects. In the carnivorous birds, the intestinal canal is much shorter than in those living upon a vegetable or mixed diet. Their food is capable of more rapid digestion and introduction into the circulation, and, as a necessary consequence, the organ which regulates the amount of the solid and fluid materials of the blood, and eliminates all waste and useless matters, must be correspondingly larger.

According to our investigations, the average weight of the kidneys of birds is relatively greater than that of mammalia. That this should be the case, might be expected from the constitution of birds. Their circulation is more rapid than that of the mammalia; the temperature of their bodies is some ten degrees higher, and their mode of life is, in all respects, more active and energetic. They consume relatively more food, and the skin is not so active in its functions. The necessary consequence of all these actions is, that the materials of their bodies will more rapidly undergo change, and need a corresponding rapid supply of nutriment, and organs adequate to perform the offices of secretion and excretion.

The weight of the turkey-buzzard is not more than seventy times the weight of its kidneys. Here we have a bird which lives exclusively upon animal food, which gorges itself on all occasions with an unnecessary amount of food: the consequence is that it must have large organs, capable of eliminating from the blood a large amount of useless materials.

3. Reptilia.—The kidneys of reptiles are situated, like those of mammalia and fish, in contact with the posterior walls of the abdominal cavity, and lie upon each side of the vertebral column. In this class they are always lobu-

lated, but differ widely as to length, breadth, and general form, and position in the different orders.

In the chelonia and batrachia, their general form may be described as elongato-oval. Fig. 3 represents the kidney of the emys terrapin, natural size.

In the sauria they are triangular, broad, and thin, with numerous convoluted lobules. Fig. 4 represents accurately the kidneys of the *Alligator Mississipiénsis*, reduced to one-ninth the natural size. Fig. 5 is the kidney of the six-striped lizard (*Ameiva Sexlineata*).

Fig. 3.

Fig. 4.

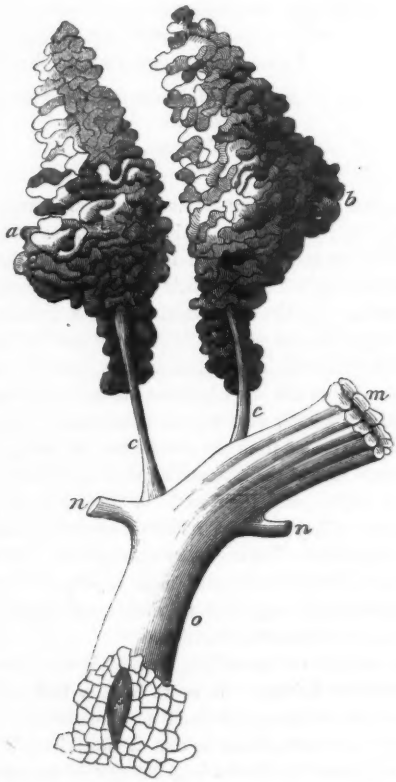


Fig. 5.

Fig. 3. Kidney of the emys terrapin, natural size.

Fig. 4. Kidneys of the *Alligator Mississipiénsis*, one-ninth the natural size. Length of alligator six feet. *a*. Right kidney. *b*. Left kidney. *c*, *c*. Ureters. *m*. Rectum. *n*, *n*. Oviducts. *o*. Cloaca. *p*. Anus.

Fig. 5. Kidney of the six-striped lizard (*Ameiva Sexlineata*), natural size.

In the ichthyodea and tailed batrachia they are narrow, and run to a point anteriorly.

In the ophidia they are narrow and much elongated, and often situated asymmetrically. In this order, the effect of the general form of the animal upon that of its kidneys, and in fact upon all the internal organs, is very perceptible. In the coachwhip snake (*Psámmophis Flagelliformis*), a long, slender serpent, constructed for swift motion, the kidneys are correspondingly long and slender, and are situated asymmetrically, the one above the other, the abdominal cavity not being sufficiently capacious for both, lying parallel. This form and arrangement of the kidneys may be said to be due, primarily, to the habits and mode of life of this snake. It subsists upon *lizards* and insects, which are active and swift. Their enemy must be correspondingly constructed for activity and swiftness of motion. The same observations apply to the black-snake (*Coluber Constrictor*) and other reptiles having the same habits.

The rattlesnakes, bull-snake (*Pituophis Melanoleucus*), and others, which live by force and stratagem, rather than by activity, have a large stout body, and correspondingly broad and thick kidneys. Fig. 6 represents the kidneys of the coachwhip snake (*Psámmophis Flagelliformis*) reduced to one-fourth their natural size.

Fig. 6.

Kidneys of the coachwhip snake (*Psámmophis Flagelliformis*), one-fourth the natural size. *a.* Large intestine. *b.* Right kidney. *c.* Left kidney. *d.* Right ureter. *g.* Left ureter. *m, m.* Inferior portions of the ducts of the ovaries. *n.* Cloaca. *o.* Ventral scales. *p.* Anus.

In the ophidia and sauria, a urinary bladder is absent, the ureters open directly into the cloaca, as may be seen in Figs. 4 and 6.

The chelonia have a large bladder, and the ureters, instead of penetrating its walls, empty themselves directly into the cloaca in front of it, so that the urine has to reascend to the urinary reservoir.

The ureters in ophidia are situated upon the internal inferior edges of the kidneys.

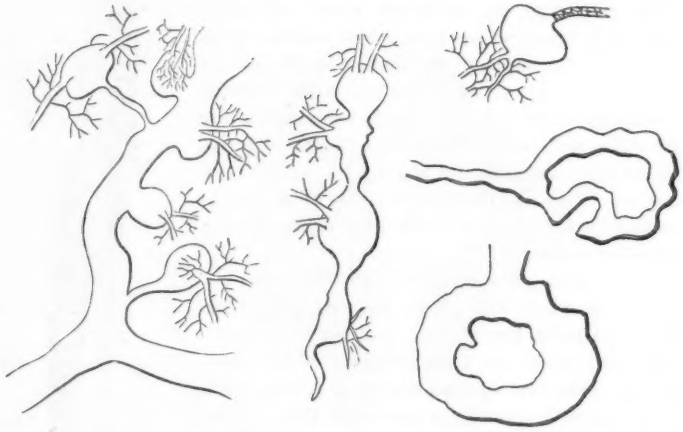
The supra-renal capsules are found in the sauria, chelonia, and ophidia, and are generally of a yellow colour, and elongated, lobulated form, and situated above the kidneys, either within the folds of the peritoneum, or more freely near to the sexual organs.

a. Minute Anatomical Structure.—The essential structure of the kidneys is nearly the same throughout all the orders of reptiles. The distinction between the cortical and medullary substance does not exist in this class.

The ureter, which runs along the entire length of the organ, sends off numerous canals, which are short in the lower orders, and long and convoluted in the higher. These urinary tubes form groups constituting lobules, which receive branches from the portal trunk that supplies the organ with blood. In the animals of this class, whose kidneys we have examined under the microscope, we have invariably found the tubuli uriniferi terminating in club-shaped extremities, loops, and Malpighian corpuscles.

Figs. 7, 8, and 9, represent a magnified view of the tubuli uriniferi of

Fig. 7. Fig. 8. Fig. 9.



Tubuli uriniferi, and corpuscles of Malpighi, from the kidney of the glossy black pine-snake (*Pituophis Melanoleucus*). Magnified about 190 diameters.

the glossy black pine-snake (*Pituophis Melanoleucus*), which were injected with the chromate of lead. It will be seen, from this injection, that the dilatations of the urinary tubes occur all along their course, the Malpighian corpuscles being not restricted, as in mammalia, to the cortical substance, but distributed throughout the entire substance of the kidney. The ciliary motion has been discovered, by Bowman and other observers, in the neck of the Malpighian bodies, and in the commencement of the tubuli uriniferi of serpents, and also of the frog, salamander, triton, bombinator, and bufo. The ciliary motion is in the direction of the urine, towards the ureter. The utility of this arrangement is evident, when we consider that in many of these animals, as the ophidia, the urine is in a semi-solid condition, containing numerous globules of the urate of ammonia.

b. Relative Size of the Kidneys of Reptiles.—The kidneys of the sauria and ophidia are larger than those of the chelonia and batrachia. The

former are carnivorous, and much more voracious than the latter. The kidneys of the ophidia are relatively much larger than those of either mammalia or birds. The body of the pine-snake (*Pituophis Melanoleucus*) was found to be not more than sixty times as heavy as its kidneys. The kidneys of carnivorous birds and mammalia are not relatively so heavy.

The question immediately arises, why should cold-blooded animals, whose circulation is sluggish, who eat but seldom, often abstaining from food for months, and even years, and whose habits are indolent, be provided with large urinary organs? In birds, we have seen that an elevated temperature, rapid circulation, and active, energetic mode of life, called for large organs, capable of removing in an efficient manner from the blood the products of the disintegrations of the system; and here we have a condition of things, directly contrary in every respect, calling for still larger organs to perform the same office.

In attempting to explain this phenomenon, we must consider the following facts and laws of the animal economy. The blood contains a definite amount of materials to be eliminated by the kidneys, diffused through its entire mass. That all these materials should be separated, the entire mass of the blood must be presented to these organs; and the rapidity of the elimination will depend upon the rapidity of the circulation, the activity of the excreting cells, and the size of the organs.

The temperature exerts a great influence upon all the actions of the animal economy. It is a well-known fact that seeds will not germinate or eggs hatch; or, in other words, life action will not commence or continue below a certain temperature. If the temperature of the cold or warm-blooded animals be reduced much below the normal standard, their functions will gradually become more feeble, and finally cease altogether. It may be stated, as a general law, that the lower the temperature of any organ, the more feeble will be its action. Nervous force exerts a great influence upon secretion. It is known to every naturalist that cold-blooded animals have much less muscular and nervous power, in proportion to their size, than the warm-blooded animals.

From these considerations, then, it is evident that to perform the same office, to secrete precisely the same amount, larger organs are required in the cold than in the warm-blooded animals.

The character of the alimentary canal, and the habits of serpents, also render the existence of large kidneys necessary. These animals are carnivorous, and have a short alimentary canal, which, in many instances, is almost completely straight. They take their food in large quantities, often swallowing animals heavier and larger than themselves. All this animal matter is capable of digestion and absorption into the blood, a large portion of which is superfluous. The great channel for the elimination of this is the kidneys; the lungs being mere sacks, and performing the function of respiration in an imperfect manner.

4. Pisces.—In the different orders of this class, we find many diversities in the general form and situation of the kidneys. There is no special type, and the development of the kidney does not, by any means, correspond with the position which the fish occupies in the classification of naturalists. The kidneys of fish of a low order, as the stingray, resemble, in position, structure, and appearance, those of the chelonia and batrachia, and appear, in more respects than one, to be of a higher organization than the kidneys of many of the acanthopterygii and other fish occupying relatively much higher ranks. We will give a few examples, to show the general position, character, and diversities of the kidneys of different fish.

The kidneys of the garfish (*Lepisosteus Osseus*) commence immediately beneath the heart and opening in the œsophagus (through which air passes into the lung), on each side of the inferior portion of the cranium, and extend throughout the length of the abdominal cavity to within about one-fourth of an inch of the anus. Their posterior surface is in contact with the vertebral column and the posterior walls of the abdominal cavity, while their anterior surface lies in contact with the posterior surface of the lung. They are covered with dense fibrous tissue, which is reflected from the lung over the kidneys, and unites with the fascia of the posterior walls of the abdominal cavity.

The vertebræ upon their anterior face have a prominent central keel, which is perforated. Through this perforation pass portions of the kidneys on each side, and coalesce together. In this respect, the kidneys of this remarkable fish, the connecting link between reptiles and fish, differ from all others that we have yet examined. The kidneys fill up the depressions in the vertebra, and those in the posterior walls of the abdominal cavity between the ribs; they are therefore lobulated.

Their length is very great, whilst their breadth is comparatively small, as will be seen in the following measurements:—

Length of fish	$2\frac{1}{2}$ feet.
Length of each kidney	12 inches.
Breadth " "	$\frac{3}{10}$ths of an inch.

A section of the kidney, showing its position, appearance, and the perforation of the keel, is shown in Fig. 10.

The ureters run along the edges of the kidneys next to the vertebral column. They dilate into a bladder, which is situated posteriorly to the rectum.

The lung extends from an opening in the œsophagus, throughout the length of the abdominal cavity. It is capable of great distension, having, when inflated, a diameter almost equal to that of the fish.

The salt-water catfish (*Galeicthys Marinus*), belonging to the same order as the garfish, malacopterygii abdominales, has both kidneys coalesced into one; thus differing entirely in appearance, relative dimensions, and position,

from those of the garfish. The general shape of the body of the kidney may be said to be semi-lunar. Superiorly it is wide, and gradually tapers until it

Fig. 10.

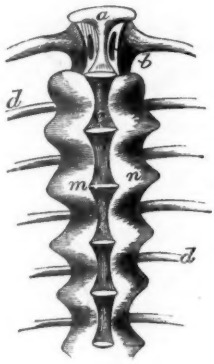


Fig. 11.

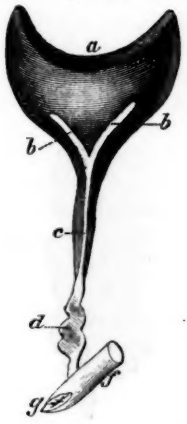


Fig. 10. Section of the kidneys of the garfish (*Lepisosteus Osseus*). Natural size. *a*. Articulating surface of the vertebra. *b*. Aperture in the anterior central prominent portion or keel of the vertebra, through which the kidneys pass and coalesce with each other. *c*. Anterior surface of the vertebral keel. *d, d*. Ribs. *m*. Right kidney. *n*. Left kidney.

Fig. 11. Kidneys of the salt-water catfish (*Galeichthys Marinus*). Natural size. Length of fish 10 inches. *a*. Superior concave border of the kidneys, which receives the inferior convex border of the air-bladder. *b, b*. Right and left branches of the ureter. *c*. Ureter. *d*. Bladder. *f*. Rectum. *g*. Anus.

reaches the bladder. The superior border is concave, and embraces the posterior convex border of the air-bladder. Fig. 11 will give a much more accurate idea of its appearance than any description.

In many of the acanthopterygii, as the whiting (*Umbrina Alburnus*), the yellowtail (*Leiostomus Zanthurus*), the young drum (*Pogonias Fasciatus*), the croker (*Micropogon Undulatus*), the sheephead (*Sargus Ovis*), and the angelfish (*Ephipphus Faber*), the general position and arrangement of the kidneys are similar, in most respects, to those of the garfish. The toadfish (*Batrachus Tau*), placed by naturalists in this order, has kidneys differing in position, size, appearance, and, in fact, in every respect from these individuals. They are short, thick, elongato-oval, resembling, in every respect, the same organs in the plagiostomi, fish of a much lower organization, according to the classification of naturalists, and also those of many reptiles, as the chelonia, sauria, and batrachia.

The kidneys of the stingray (*Trygon Sabina*), resemble, in structure, appearance, and situation, those of the chelonia and batrachia, and, among fishes, those of the toadfish. Fig. 12 represents the kidney of the stingray. By comparing this with the kidney of the emys terrapin (Fig. 3), or those of the alligator (Fig. 4), the resemblance will immediately be seen.

Fig. 13 represents the kidneys of the whiting (*Umbrina Alburnus*), reduced one-third their natural size.

Fig. 12.

Fig. 13.

Fig. 12. Kidney of the stingray (*Trygon Sabina*).

Fig. 13. Kidneys of the whiting (*Umbrina Alburnus*). One-third the natural size. Length of fish 12 inches. *a*. Inferior portion of the cranium. *b*. Left kidney. *c*. Right kidney. *g*. Muscular attachment of the œsophagus to the vertebral column. *f*. Vertebra. *o, o*. Ureters. *p*. Bladder lying posterior to the rectum. *m*. Rectum. *n*. Anus.

This may be taken as the type of the kidneys of nearly all the fish belonging to the order Acanthopterygii. By comparing with each other Figs. 10, 11, 12, and 13, we will see at once that there is no definite type of kidneys for each order, or for fish as a class. A bladder is present in many individuals of this class, and in all cases it lies posterior to the rectum, differing in this respect from the arrangement of the bladder in mammalia and reptilia, and resembling in its position the rudimentary bladder of birds. This one fact will serve to distinguish fish from all other animals.

The supra-renal capsules are very small in fishes. In some orders, they are small, bean-shaped bodies, of a pale colour, connected by vessels with the apex of the kidneys. In others, they are small, reddish-white corpuscles, placed behind the kidneys, against the vertebral column.

a. Minute Anatomical Structure.—The kidneys of fish consist of tufts of urinary tubes, which are sent off transversely at intervals from the long ureter, and are connected together by the loose fibrous tissue which supports the network of vessels distributed upon their walls. When portions of the kidney of the garfish (*Lepisosteus Osseus*) were carefully viewed under the microscope, numerous tubuli uriniferi were seen, dividing and subdividing into dis-

tinct branches, in some cases terminating in loops, in others, in club-shaped dilatations, and in others still, in cup-like dilatations, which embraced convoluted masses of bloodvessels. These corpuscles of Malpighi, although small, were distinct and well formed. The urinary tubes in most fish are long and tortuous, but seldom ramified. The corpora Malpighiana are generally present; they are, however, in some of the lower orders, as the cyclostomi, imperfectly formed. The minute anatomical structure of the kidneys of fish reminds us of that of the corpora Wolffiana, the temporary renal organs of the higher vertebrata.

b. Comparative Size of the Kidneys of Fish.—Owing to the great difficulty of separating, thoroughly and accurately, the kidneys from the vertebral column, to which, in most individuals, they closely adhere, this subject has not been investigated far enough to allow of general conclusions.

The following weights are not without some interest:—

Weight of catfish, 90 times heavier than that of its kidneys.
" " toadfish, 114 " " " " " "
" " stingray, 177 " " " " " "

II. *Comparative Anatomy of the Kidney in the Invertebrate Animals.*

In this class, the kidneys, like all the other organs, are much more simple in their structure than those of the vertebrata. The following are the most important results of the recent researches of Siebold and other anatomists.

1. Arthropoda.—In the class insecta, the kidneys consist of small tubes, varying in length, number, and modes of grouping in the different orders. These tubes open upon the posterior or pyloric extremity of the stomach, by means of one or two common excretory ducts. These ducts are sometimes dilated, bladder-like, at their point of insertion. The free extremities of the uriniferous tubes generally terminate cœcally or anastomose with each other. When very long, they form numerous convolutions around the digestive canal, and sometimes creep with their anterior extremities between the coats of the stomach, or by their posterior between those of the colon. They are composed of a homogeneous external tunic or basement membrane, filled internally with large cells, disposed in rows, so that nowhere can there be perceived in the interior of the vessel a glandular canal defined by a special epithelium. Each cell contains a colourless nucleus, and a multitude of very fine yellow or brown granules. These cells are ruptured, and the granular bodies, which constitute the urinary secretion, pass through the spaces between the cells, and flow gradually into the intestinal canal. The urinary organs of the larvæ and pupæ of this class resemble somewhat those of the perfect insect.

The urinary organs of the arachnoidæ consist of small, usually multiramose glandular tubes, which open into the cloaca, and exactly resemble the Malpighian vessels of the insecta, both by their structure and the nature of the fluid which they secrete.

In the crustacea, up to the present time, urinary organs have been observed only in the myriapoda, and like those of the insecta, they consist of very small, brownish, cœcal vessels, describing many convolutions around the stomach and intestines, and open into the digestive canal at the boundary between the stomach and intestine.

2. Mollusca.—The urinary organs of the cephalopoda present the appearance, externally, of variously ramified glandular tufts, connected with the venæ cavæ, and sometimes with the principal veins. Careful examinations have shown these organs to consist of an internal parenchyma, composed of a contractile fibrous tissue, through which are spread branches coming from the venæ cavæ. This parenchyma is surrounded by a structureless membrane, covered with several layers of nucleated granular cells. The urine, of a dirty yellow colour, is secreted from the external surface of this cellular layer, and escapes immediately into the peritoneal cavities, and then is discharged externally through their orifice. The peritoneal cavities in this function resemble urethral canals.

With most of the cephalophora, the kidney consists of an uneven lamellate gland, which is usually situated near the bronchial or principal pulmonary vein. The excretory duct accompanies the rectum, and often opens near the anus. The gland is surrounded by a sac-like envelop, which is continuous with the internally ciliated excretory duct. Upon the exterior of this membranous envelop, which appears to act the part of a basement membrane, ramify bloodvessels. In the gland itself, no bloodvessels have been observed. Each renal lamella is composed of thickly-set, delicate cells, loosely bound together, containing a transparent liquid, in which float brown or violet-coloured nuclei. These nuclei are in form round and embossed; they have a dense crystalline structure, and are undoubtedly a product of renal secretion, because they contain uric acid.

A very remarkable organ, found in all the lamellibranchia, known as the gland of Bojanus, has been demonstrated to be undoubtedly of a renal nature. This organ is always double, and consists of a large, long sack, with glandular walls of a dirty yellow or dark green colour. It is situated on each side of the back, between the pericardium and the inferior abductor muscle, and extends usually upon the sides of the abdomen to the base of the branchiæ. These glands communicate with the cavity of the mantle, by two small openings, which have swollen borders, and are situated sometimes at the upper and sometimes at the lower end of the sac. The usually very thin walls of these two sacs have numerous folds or plicæ, which form compartments or areolæ, all of which are covered with a very delicate ciliated epithelium. The parenchyma of these walls is composed of a very loose tissue, which, upon the least disturbance, separates into small granular cells. Many of these cells contain a blue-black, round nucleus, which is very solid, and sometimes so large as to be visible to the naked eye as an inorganic concretion.

As these nuclei contain uric acid, they may be compared to renal calculi. The walls of these kidneys are surrounded by a distinct network, which arises from the large venous reservoir in which the afferent blood of the body is accumulated.

No special organs for the excretion of the urine have as yet been discovered in the vermes, zoophyta, or protozoa.

III. *The Excretions of the Kidney in the Vertebrata and Invertebrata.*

The offices of the kidney in the animal economy are—

1. To separate from the blood those products of the breaking-down and metamorphosis of the tissues which can be of no further use in the animal economy, and which, if retained, would be productive of evil consequences.
2. To remove an excess of nutritive materials, when they have been introduced into the blood in an amount beyond what is necessary to supply the wants of the animal economy.
3. To draw off from the blood any extra, unnecessary amount of water.

The active agents in these processes, in all animals, both vertebrate and invertebrate, are the epithelial cells lining the basement membrane of the tubuli uriniferi, or the renal sacs. These cells possess a power of separating certain materials, as urea and uric acid, from the blood, whilst they allow its other constituents to pass untouched. That this power of depurating the blood is resident in them alone, is conclusively demonstrated by the fact that, when they are removed from the basement membrane of the tubuli uriniferi, immediately albumen and other constituents of the blood, not normally excreted by the kidneys, appear in the urine. The basement membrane of the urinary tubes, then, does not possess this power of selection. The power which these cells possess, of preventing certain materials of the blood from passing through them, is definite, and capable of being overcome by pressure. When the walls of the heart are preternaturally thickened, so that the blood is propelled with great force through the circulatory apparatus, it often happens that the power of selection in the cells is overcome, and albumen and other necessary constituents of the blood are forced out along with the excrementitious matters, and appear in the urine.

The distinction between an excretion and a secretion should here be drawn. These acts have been confounded by almost every physiologist.

The circulatory apparatus not only carries nutriment to the different organs and tissues, but also removes from them the products of their disintegration and metamorphoses, which can be of no further use in the animal economy. The act of separating these materials from the blood is called excretion. The process is one of simple separation. The amount and character of an excretion depend entirely upon the amount and character of the excrementitious materials existing in the blood.

If the kidneys be extirpated, their office will be assumed by the skin, and even the gastro-mucous membrane. In like manner, if the function of the skin be checked, it will be assumed by the kidneys. The act, then, of separating certain materials from the blood can be transferred from one excretory organ to another.

A secretion does not pre-exist in the blood. We do not find the gastric juice or the salivary fluid existing in the blood before they are elaborated by special organs. Each secretion must have a special set of cells, which alone can produce the peculiar material. We never find one organ elaborating the secretion of another distinct organ. This is a fixed law. The salivary gland never secretes gastric juice, nor the mammillary gland bile. The stomach of a rattlesnake never secretes the deadly fluid of its poison-gland. Thus, in two essential respects, an excretion differs from a secretion.

In both excretion and secretion, the active agents are organic cells. Under the microscope, these cells present very much the same appearance; there are no distinctive characters corresponding to their different offices. The most accurate means of investigation which we possess have failed as yet to give us any satisfactory interpretation of a great phenomenon, which lies at the foundation of all our knowledge of living beings. How do organic cells elaborate particular products, or separate certain materials from the blood? Medicine can never, in truth, be considered a science until this question is definitely settled.

A series of experiments, which I performed last summer, throws a faint glimmering of light upon this subject. Animal membranes were filled with solutions of certain chemical substances, as oxalate of ammonia, and immersed in solutions of other substances, as chloride of calcium, which would form uncrystallized, insoluble precipitates when mingled. It was found, when endosmose and exosmose of the solutions had taken place, that the precipitates were crystalline, and differed in the physical arrangement of their particles on the opposite sides of the membranes. The physical influence exerted by the membranes differed with the direction of the current. By extending the experiments, it was found that membranes from different parts of the same animal, or from relatively the same parts in different animals, effected different physical arrangements of the molecules of the same body. When live frogs were allowed to remain for a certain length of time in a solution of the oxalate of ammonia, and carefully washed and transferred for a similar length of time to a solution of the chloride of calcium, beautiful octohedral, dumb-bell, cubical, and ellipsoidal crystals of the oxalate of lime were found, by the aid of the microscope, in the blood and different tissues. Similar results were obtained when the cut ends of growing plants were treated in a similar manner. These experiments were repeated, with similar results, with various other chemical substances.[1] Here, then, we have animal and vegetable

[1] For further details of these experiments, the reader is referred to the department of "American Intelligence" in this number.—Ed.

membranes and cell-walls, both living and dead, altering the physical arrangement of the molecules of inorganic compounds. In the dead animal membranes, all vital influence was absent. This change was brought about entirely by their physical constitution. That it was not due to the presence of blood, or putrefying organic matters, or some other fluid, was proved by numerous experiments, carefully performed.

It is remarkable that the physical forms impressed both by the living and dead animal membranes upon the oxalate of lime, were precisely those which occur in the urine of various animals, and even in that of birds and fishes. Is the conversion of albumen into albuminose more wonderful, or does it require more power than this influence exerted by a dead animal membrane upon an inorganic substance? Do not the results of these experiments render it probable that many secretions and excretions in the animal economy are the results of purely physical causes? It is highly probable that membranes, by minutely dividing and separating the ultimate particles of substances passing through them, communicate to them certain motions, thus changing their molecular arrangement, and with it their physical and chemical properties.

In considering the causes which influence the physical and chemical properties, and relative amounts of the excretions of the kidney in different animals, we should investigate carefully the following subjects:—

1. The Amount, Force, and Rapidity of the Circulation of the Blood. When definite substances in limited proportions are required to be eliminated from the blood by any organ, the activity of that organ will depend upon the activity of its excreting cells; the amount of blood supplied to those cells, and the force with which it presses against them. The influence of arterial pressure upon the function of the kidneys is conclusively proved by M. Bernard's experiments. A dog having an arterial pressure of 76 millimetres, its kidneys secreted 0.80 grammes of urine in a minute. When the pressure of the blood was increased to 112, by tying all the arteries of the limbs, 12.23 grammes were secreted in the same length of time. Another animal, in whom the pressure was reduced by bleeding to 57, passed in one minute 2.06 grammes. The pressure was raised to 122 by the injection of blood into the circulation, and then 19.34 grammes of urine passed. These experiments were varied and performed in several instances, and always with the same result; the quantity of urine increased or diminished with the increase or diminution of the force of the circulation.

2. The Activity of the Glands of the Tegumentary System, which are connected with, and materially modify the function of the kidneys. They both secrete from the blood, substances closely allied in physical and chemical constitution. The quantity of these substances is limited. The sluggishness, or increased activity of the one, therefore, must necessarily produce a state directly opposite, in the other.

3. The Action of the Colon. In some animals, the colon performs an im-

portant office in the depuration of the blood, whilst in others, as reptiles, its function in this process is exceedingly feeble.

4. The Activity of Respiration. An intimate connection exists between the function of the lungs and the materials eliminated by the kidneys. Upon the function of respiration depends, in a great measure, the production of heat, and upon the temperature depend many of the chemical changes taking place in the animal economy.

5. The Physical and Vital Constitution, and Modes of Living, influence materially the size and activity of the kidneys, and the amount and character of its excretions. Birds, whose circulation is rapid, whose temperature is high, whose exercise is frequent, swift, and violent, need large kidneys, to eliminate in a corresponding manner the incessant wastes of their tissues. Serpents who seldom take food, but who, when they do, gorge themselves with large and unnecessary quantities, need an organ which will be able to eliminate this superabundant mass of nutritious matter, which has been received by absorption into the blood. In cold-blooded animals, the temperature is low, the circulation sluggish, respiration imperfect, and, as a necessary consequence, all the operations of secretion and excretion are correspondingly slow, and require relatively much larger organs to accomplish the same amount of labor than those of the more highly organized.

1. *Urine of the Vertebrata.*

1. *Urine of the Mammalia.*—As the character of the urine is affected greatly by the nature of the food, animals may be divided into three great classes: Carnivora, Herbivora, and Omnivora. The urine of the first two, will have specific characters, whilst that of the latter will resemble both, or partake of the characters of one or the other exclusively, according to circumstances.

a. Urine of the Carnivora.—The urine of this class of animals, as a general rule, is of a much higher specific gravity, and more scant in quantity than that of the herbivora, for several important reasons. Their nutriment is of a much more concentrated form, and does not contain so large an amount of fluids. Their mode of life is more energetic and active, necessarily leading to a greater wear and tear of the tissues. And also the glands of their tegumentary system are not so numerous or active. Hieronymus examined the urine of lions, tigers, and leopards, and found it to be alike in all.

At a temperature of 50° F. to 55° F., the specific gravity of the urine from the same animal varied from 1059 to 1076.

The mixed urine of these three animals gave the following result:—

Water	846.00
Solid constituents	154.00
Urea, alcohol-extract, and free lactic acid	132.20
Uric acid	0.22
Vesical mucus	5.10

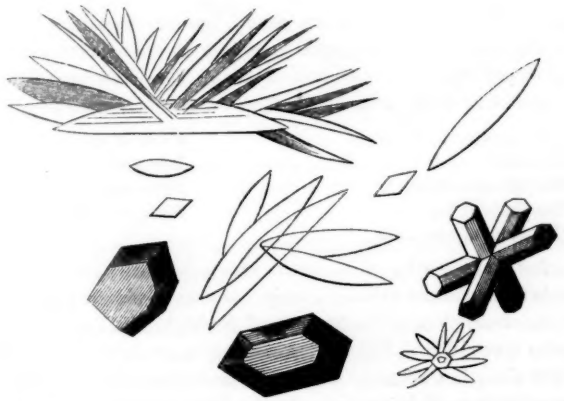
Sulphate of potash	1.22
Chloride of ammonium, and a little chloride of sodium .	1.16
Earthy phosphates	1.76
Phosphates of soda and potash	8.02
Phosphate of ammonia	1.02
Lactate of potash	3.30

Hieronymus also examined the urine of the hyena and panther, and found them to contain the same ingredients.

Specific gravity of the urine of the hyena at 50° F. . .	10.61
" " " " panther at 48° F. . .	10.45

The following is a qualitative analysis of the urine of the red bat (*Nycticeus Noveboracensis*), which I made during the month of August. This animal being very small, only a few drops could be obtained from its bladder, consequently our instrument in its analysis was the microscope. After standing for a short time upon a glass slide, there made their appearance, under a magnifying power of 210 diameters, conglomerated masses of lozenge-shaped crystals of uric acid, small globules of the urate of ammonia, prismatic crystals of triple phosphate, and a remarkable looking crystal, composed of three hexagonal columns, crossing each other at right angles. This crystal revolved over and over in the urine upon the slightest motion. Fig. 14

Fig. 14.



Crystals of uric acid and triple phosphate deposited in the urine of the red bat (*Nycticeus Noveboracensis*), a short time after its removal from the bladder. Magnified 210 diameters.

represents these crystals. When treated with hydrochloric acid, cubical and octohedral crystals, and crosslets of the chloride of sodium, and lozenge-shaped crystals of uric acid made their appearance under the microscope. In a speci-

men of urine from another red bat, innumerable beautiful prismatic crystals of the triple phosphate were deposited a short time after it was taken from the bladder.

These are represented in Fig. 15. When treated with a drop of concentrated nitric acid, the whole mass of urine immediately became solid, pre-

Fig. 15.

Crystals of triple phosphate, deposited in great number in the urine of the red bat, when slightly evaporated. Magnified 210 diameters.

senting to the naked eye a white silvery, opaque, crystalline appearance. Under the microscope, it was seen to consist of a conglomerated mass of lozenge-shaped crystals of the nitrate of urea. The following are the principal constituents of the urine of the *Nysticeus Noveboracencis*:—

Urea, in great abundance.
Uric acid.
Chloride of sodium.
Phosphate of lime.
Phosphate of ammonia.
Urate of ammonia.
Purpurine.
Phosphate of soda.
Phosphate of magnesia.

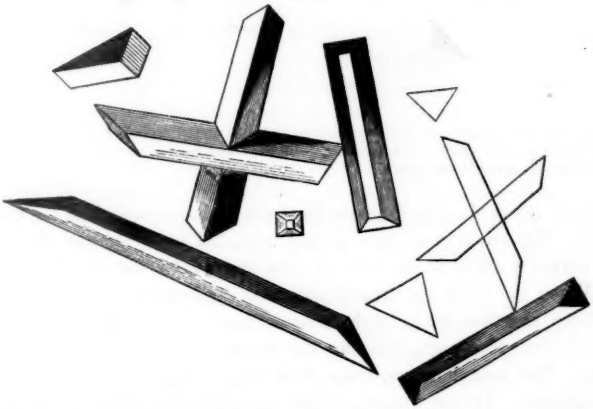
The fact that the urine of the bat contains so large an amount of urea may be due in some measure to its high temperature and rapid circulation. According to the observations of Tiedemann and Rudolphi, the temperature of the bat varies from 106° to 105° F. According to another observer, its heart beats 200 times in a minute. This rapid circulation must be attended by a correspondingly rapid disintegration of the fluids and tissues.

It is to be regretted that specimens of the urine of wild carnivorous animals can rarely be obtained. This is the case with not only the small, but also the large animals. From the bladders of ten raccoons and one mink, I was unable to obtain a single drop of urine for microscopical or chemical examination.

b. Urine of Herbivora.—In the urine of animals of this class, uric acid is replaced by hippuric acid. The amount of the solid constituents, in proportion to that of the water is much less, and the specific gravity, as a necessary consequence, much lower than in the urine of carnivorous animals.

Qualitative Analysis of the Urine of the Gray Squirrel (*Sciurus Carolinensis*).—Owing to the small capacity of the bladder of this animal, it is impossible to obtain a sufficient quantity of urine for a quantitative analysis. The urine is of an amber colour. When allowed to stand for a short time after its removal from the bladder, beautiful prismatic crystals of the triple phosphate were deposited, and were visible to the naked eye as a silvery-white precipitate. Fig. 16 represents a deposit of very long and delicate crystals of this salt.

Fig. 16.



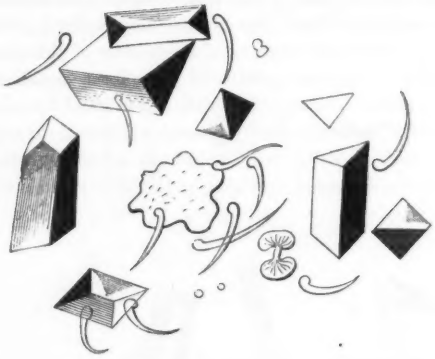
Prismatic crystals of triple phosphate, precipitated from the urine of the gray squirrel (*Sciurus Carolinensis*), which was placed upon a glass slide, and allowed to remain for one hour. Magnified 210 diameters.

Under the microscope, these differed somewhat in form in different individuals. In several instances, two of the prisms, as represented in the figure, were united so as to form a cross.

Fig. 17 represents the same deposit which took place in the urine of several squirrels collected in a test-tube. It was rutting season, and the spermatozoa are distinctly seen, many of them adhering to the crystals. A well-formed dumb-bell crystal, of the oxalate of lime, is also seen. The precipitation of these crystals results from the formation of ammonia, one of the products of the disintegration of the nitrogenized constituents of the urine. The ammonia thus formed neutralizes the small amount of free acid in the urine, which renders the triple phosphate soluble. When the urine was treated with aqua ammonia, stellate crystals of this salt made their appearance in great numbers.

The presence of urea was determined by slightly evaporating a portion of the urine, and treating it with a drop of concentrated nitric acid; this was

Fig. 17.



Prismatic crystals of triple phosphate, spermatozoa, and a dumb-bell crystal of the oxalate of lime, deposited from the urine of several gray squirrels, which was placed in a test-tube and allowed to remain for two hours. Magnified 210 diameters.

followed by the production of numberless lozenge-shaped crystals of the nitrate of urea. The quantity of urea present appeared to be greater than that which normally exists in the urine of man, and smaller than that of the red bat. When the urine was treated with nitric acid and evaporated over the spirit lamp, and then treated with ammonia, but a slight change to the purplish-red colour took place, denoting the presence of uric acid in very small amount. This acid, however, exists in a larger proportion in the urine of some squirrels. In one instance, where the urine was spread upon a glass slide and slowly evaporated, crystals of uric acid made their appearance in moderate abundance.

The presence of hippuric acid was rendered evident, by slightly evaporating the urine and treating it with acetic or hydrochloric acids. Under the microscope, it presented the form of delicate acicular crystals. When the urine, thus treated with hydrochloric acid, was still further evaporated, chloride of sodium crystallized in cubes and crosslets. In the urine of one individual, which was spread upon a glass slide and allowed slowly to evaporate, hippuric acid made its appearance in considerable abundance. The following are the principal constituents of its urine:—

Urea.
Uric acid, in small amount.
Hippuric acid.
Soda, probably combined with hippuric and uric acids.
Oxalate of lime.
Phosphate of lime.
Phosphate of ammonia.
Phosphate of magnesia.

Qualitative Analysis of the Urine of the Wood Rat (*Néotoma Floridána*).—This animal lived about a corn-house and stable. When the urine was simply spread upon a glass slide and allowed slowly to evaporate, prismatic crystals of triple phosphate, numerous acicular and arborescent crystals of hippuric acid, and cubes and crosslets of chloride of sodium, successively made their appearance under the microscope. When treated with nitric acid, heat, and ammonia, no trace of uric acid was discovered. Urea appeared to be present only in small quantities. The addition of aqua ammonia precipitated stellate crystals of triple phosphate. The following are its principal constituents:—

Urea, in small amount.	Chloride of sodium.
Hippuric acid.	Phosphate of magnesia.
Phosphate of lime.	Phosphate of ammonia.

Qualitative Analysis of the Urine of the Common Rabbit (*Lépus Sylváticus*), slightly alkaline in its reaction. Specific gravity 1020.—When first deposited, the urine presented a turbid, milky appearance. After standing for a time, a white, chalk-like precipitate settled to the bottom. Under the microscope, this deposit, in some instances, consists of minute globules often conglomerated together, resembling, in all respects, the urate of ammonia; in other cases, it consists of large globules. That this deposit does not contain urate of *ammonia*, or any uric acid whatever, is proved by the fact that it gives no purplish-scarlet colour when treated with nitric acid, heat, and aqua ammonia.

When mixed with the hydrated oxide of potassium, no ammonia was evolved; and, when heated in water, it was not dissolved. When treated with nitric, hydrochloric, or acetic acids, effervescence takes place, and the deposit dissolves, and, under the microscope, numerous acicular crystals of hippuric acid make their appearance. By close microscopical and chemical investigation, this deposit was found to contain the hippurate of soda. In the deposits, which consisted of large globules, were found dumb-bell crystals of the oxalate of lime.

The following are the principal constituents of the urine of the *Lépus Sylváticus*:—

Urea.	Oxalate of lime.
Hippurate of soda.	Phosphate of lime.
Carbonate of soda.	Phosphate of magnesia.
Carbonate of lime.	Phosphate of ammonia.

Quantitative Analysis of the Urine of the Hare.—The urine of this animal has been examined on two occasions by Von Bibra, who collected three pints from seven or eight hares, by external pressure on the region of the bladder. The first analysis was made in December. Hippuric acid was found to be present in small quantity, forming 0.007% of the urine.

The ash contained:—

Chloride of sodium, with a little chloride of potassium	7.12
Sulphate of soda	16.82
Carbonate of soda	9.84
Phosphate of soda	53.05
Phosphates of lime and magnesia	13.17
	100.00

The following is the analysis of the urine obtained in a similar manner in the month of June. Faint alkaline reaction. In the course of six hours, crystals of ammoniaco-magnesian phosphate were observed upon the surface. Specific gravity 1050; 1,000 parts contained:—

Water	912.86
Solid constituents	87.14
Urea	8.54
Hippuric acid	microscopic crystals.
Alcohol-extract	9.58
Water-extract	32.68
Soluble salts	23.70
Insoluble salts	12.64

The ash consisted of—

Chloride of sodium, with a little chloride of potassium	22.49
Sulphate of soda	29.97
Carbonate of soda	8.73
Phosphate of soda	4.39
Phosphate of lime	12.00
Phosphate of magnesia	22.42
	100.00

The urine of Guinea-pigs resembles in many respects that of the hare, and herbivorous animals generally.

The urine of beavers, according to the qualitative analysis of Vauquelin, contains carbonate of lime and magnesia, urea, hippurate of soda, and other ordinary ingredients of the urine of herbivorous animals, but no phosphates or uric acid. He also detected in the urine the undecomposed colouring matter of the willow, one of the constituents of the ordinary food of the beaver.

The urine of elephants has been examined by Vogel, who found it turbid from carbonates of lime and magnesia floating through it. According to his analysis, it contained no trace of hippurate of potassa, but, on the other hand, urea is more abundant than in that of the rhinoceros. Brandes, who afterwards examined the urine of elephants, found hippuric acid, combined both with an alkali and urea.

The urine of the rhinoceros was also examined by Vogel. He found it turbid and yellow, resembling clay-water mixed with ochre, having a peculiar smell, not unlike that of mashed ants. When exposed to the air, it becomes

darker, and is covered by a pellicle of small crystals of carbonate of lime. It effervesces with acids, and becomes clear. When allowed to stand, it deposits a yellow powder, amounting to 2¾ per cent. of the urine, and consists of the carbonates of lime and magnesia, combined with a nitrogenized animal matter mixed with sesquioxide of iron and silica. The addition of hydrochloric acid to the urine, previously evaporated to one-third of its volume and filtered, precipitates hippuric acid to the amount of 0.45 per cent., which was previously combined with potassa. The urine of this animal also contained urea and the ordinary salts.

The urine of the pig has been analyzed by Van Setten, Von Bibra, Boussingault, and Lassaigne. Considerable and important discrepancies exist between the different analyses, which may be due to various causes, as difference of food, relative amounts of fluids consumed by the animals, time of the year, &c. The following is the analysis of Van Setten. Urine, colour yellow, inodorous; specific gravity 1003; 1,000 parts contained—

Water	990.028
Solid constituents	9.972
Urea	0.750
Uric acid	0.195
Water-extract	1.708
Alcohol-extract	1.105
Resinous matter	0.425
Albumen and mucus	0.721
Lactic acid	0.490
Stearin	0.092
Sugar	0.375
Phosphate of soda	1.376
Sulphate of potash, chlorides of sodium and potassium	2.075
Sulphates of lime and magnesia	0.425
Sulphate of ammonia	0.196
Chloride of ammonium	0.010

The following is an analysis of the urine of a pig feeding upon potatoes and water, slightly impregnated with salt, by Boussingault. Urine alkaline, limpid, and of a pale-yellow colour; specific gravity 1013.6; 1,000 parts contained—

Water and indeterminate organic matter	979.14
Urea	4.90
Bicarbonate of potash	10.74
Carbonate of magnesia	0.87
Carbonate of lime	traces.
Sulphate of potash	1.98
Phosphate of potash	1.02
Chloride of sodium	1.28
Alkaline lactates	undetermined.
Hippuric acid	absent.
Silica	0.07

In all the analyses of the urine of the pig, that have thus far been published, hippuric acid is absent. It is a little remarkable that this acid should not be present in the urine of animals living entirely upon a vegetable diet.

The urine of the horse has been analyzed by Fourcroy, Vauquelin, Chevreul, Simon, Boussingault, and Von Bibra. It is of a straw-colour, not unfrequently turbid; at first acid, but soon becomes ammoniacal, and then emits a peculiar, penetrating odour. It deposits, on standing, a mixture of carbonate of lime and magnesia. The specific gravity varies, with different individuals, from 1030 to 1050, according to their life, whether one of labour or quietude, and also with the relative amounts of fluids and solids consumed. The mean specific gravity of the urine of horses used for agricultural purposes may be stated to be about 1045.

The following analyses of Von Bibra and Boussingault are the most recent and perhaps the most correct.

In two analyses of the urine of the same horse, made at different periods, Von Bibra found—

	(1)	(2)
Water	885.09	912.84
Solid constituents	114.91	87.16
Urea	12.44	8.36
Hippuric acid	12.60	1.23
Water-extract	21.32	19.25
Alcohol-extract	25.50	18.26
Mucus	0.05	0.06
Salts, soluble in water	23.40	40.00
Salts, insoluble in water	18.80	

The individual salts in one hundred parts of saline residue, determined upon two occasions, were—

	(1)	(2)
Carbonate of lime	12.50	31.00
Carbonate of magnesia	9.46	13.07
Carbonate of potash	46.09	40.33
Carbonate of soda	10.33	
Sulphate of potash	13.04	9.02
Chloride of sodium	6.94	5.60
Silica	0.55	0.98
Loss	1.09	

Traces of iron were observed, but he was unable to determine the presence of fluorine. The deposit which falls after the urine has stood for some time was found in three analyses to consist of—

	(1)	(2)	(3)
Carbonate of lime	80.9	87.2	87.5
Carbonate of magnesia	12.1	7.5	8.2
Organic matter	7.0	5.3	4.3
	100.0	100.0	100.0

The horses were used for agricultural purposes, and fed on hay and oats.

Von Bibra states that the prevailing opinion that, by excessive work, the hippuric acid is replaced by benzoic acid, is incorrect. Benzoic acid was generally absent; and, when present, could only be recognized by the aid of the microscope.

The following is an analysis, by Boussingault, of the urine of a horse fed on trefoil and vetches. Specific gravity 1037.3; reaction alkaline; 1,000 parts contained—

Water and indeterminate matters	910.76
Urea	31.00
Hippurate of potash	4.74
Lactate of potash	11.28
Lactate of soda	8.81
Bicarbonate of potash	15.50
Carbonate of lime	10.82
Carbonate of magnesia	4.16
Sulphate of potash	1.18
Chloride of sodium	0.74
Silica	1.01
Phosphates	absent.

Boussingault, although he distilled upwards of twenty-six gallons at a single experiment, was unable to obtain a trace of the red oil which several chemists have noticed as a constituent of the urine of the herbivora, and on which the colour and odour are said to depend.

The urine of horned cattle was first examined by Rouelle, who stated that it contains urea in larger quantity than that of man, and also hippurate of soda. Exposed to the air, it deposits small crystals of carbonate of lime and magnesia.

The following is an analysis of the urine of cattle, by Sprengel. One thousand parts contained—

Water	926.24
Solid constituents	73.76
Urea	40.00
Albumen	0.10
Mucus	1.90
Benzoic acid	0.90
Lactic acid	5.16
Carbonic acid	2.50
Potash	6.64
Soda	5.54
Silica	0.36
Alumina	0.04
Oxide of manganese	0.01
Lime	0.65
Magnesia	0.36
Chlorine	2.72
Sulphuric acid	4.05
Phosphorus	0.70

Von Bibra analyzed the urine of oxen employed for agricultural purposes. The urine was of a dark yellow colour, having a peculiar odour. The specific gravity varied from 1032 to 1040.

The following analyses were made with the urine of the same animal, at different times:—

	(1)	(2)
Water	912.01	923.11
Solid constituents	87.99	76.89
Urea	19.76	10.21
Hippuric acid	5.55	12.00
Mucus	0.07	0.06
Alcohol-extract	14.21	10.20
Water-extract	22.48	16.43
Soluble salts	24.42	25.77
Insoluble salts	1.50	2.22

The saline residue contained—

Carbonate of lime	1.07
Carbonate of magnesia	6.93
Carbonate of potash	77.28
Sulphate of potash	13.30
Chloride of sodium	0.30
Silica	0.35
Traces of iron, and loss	0.77
	100.00

The food of the oxen consisted of fresh clover and a little hay. The salts of the saline residue were found to vary very much in quantity in different analyses. As a general rule, the urine of oxen may be said to contain more alkaline and less earthy carbonates than the urine of horses.

Analysis of the urine of a cow fed on after-math and potatoes, by Boussingault. When treated with an acid it effervesced, and numerous crystals of hippuric acid were deposited. Specific gravity 1040; 1,000 parts contained:—

Water and indeterminate matters	921.32
Urea	18.48
Hippurate of potash	16.51
Lactate of potash	17.16
Bicarbonate of potash	16.12
Carbonate of magnesia,	4.74
Carbonate of lime	0.55
Sulphate of potash	3.60
Chloride of sodium	1.52
Silica	traces
Phosphoric acid	absent

The urine of the goat was analyzed by Von Bibra. It was obtained from an animal confined in a stable, and poorly fed upon sour hay, &c. Urine

alkaline, clear, with a pungent peculiar odour. Specific gravity from 1008 to 1009.

In two instances, 1,000 parts contained:—

	(1)	(2)
Water	980.07	983.99
Solid residue	19.93	16.01
Urea	3.78	0.76
Hippuric acid	1.25	0.88
Alcohol-extract	4.54	4.66
Water-extract	1.00	0.56
Mucus	0.06	0.05
Soluble salts	8.50	8.70
Insoluble salts	0.80	0.40

The ash consisted of:—

Carbonate of magnesia, with a little carbonate of lime	7.3
Sulphate of soda	25.0
Chloride of sodium	14.7
Carbonate of soda with a little carbonate of potash	53.0
	100.0

In the urine of the camel, Chevreul discovered a large quantity of urea, but no uric acid; also chloride of sodium, hippurate of soda, carbonate of soda, sulphate of potash, carbonate of ammonia, a little sulphate of soda, and a trace of peroxide of iron. The phosphates, as well as the urates, were found to be absent.

c. Urine of the Omnivora.—Man is the only omnivorous animal whose urine has been carefully examined. We will not unnecessarily repeat analyses which have been performed by distinguished chemists, and are well known. We will only give a few analyses of Lehmann, which may be compared with those of the urine of other animals, and at the same time illustrate several important points, as the influence of diet upon the excretions of the kidney.

Analyses of the urine of a healthy, well-fed, young man, living upon a mixed animal and vegetable diet:—

	(1)		(2)		(3)	
Water	937.682		934.002		932.019	
Solid residue	62.318		65.998		67.981	
Urea	31.450		32.914		32.909	
Uric acid	1.021		1.073		1.098	
Lactic acid	1.496		1.551		1.513	
Water-extract	0.621		0.591		0.632	
Spirit and alcohol-extract	10.059		9.871		10.872	
Lactates	1.897		1.066		1.732	
Chlorides of sodium and ammonium	3.646	15.857	3.602	15.744	3.712	16.130
Alkaline sulphates	7.314		7.289		7.321	
Phosphates of soda	3.765		3.666		3.989	
Phosphates of lime and magnesia	1.132		1.187		1.108	
Mucus	0.112		0.101		0.110	

Under a strictly animal diet, the urine became pale, of a straw-color, limpid, and resembled in appearance and chemical constitution the urine of the carnivora. The following are two analyses of the same man, subsisting upon animal food, made on the 28th and 30th of July, the ninth and eleventh days of the experiment:—

	July 28.	July 30.
Water	909.32	933.27
Solid residue	96.68	66.73
Urea	53.79	41.65
Uric acid	1.41	1.18
Free lactic acid	2.28	1.64
Lactates	1.67	1.02
Extractive matter soluble in water	0.82	0.61
Extractive matter soluble in alcohol	4.50	3.24
Mucus	0.09	0.11
Chlorides of sodium and ammonium	5.37	3.46
Sulphates	11.51	7.08
Phosphate of soda	5.52	4.04
Earthy phosphates	3.72	2.70

Lehmann next investigated the effects of a strictly vegetable diet. The urine had a faint odour, and a decidedly acid reaction. That, passed in the morning, was of a dark-brown colour, and rapidly deposited a mucus sediment; after which, bright-red crystals of uric acid gradually separated.

The following are three analyses which were then instituted, on the ninth, tenth, and twelfth days of the experiment:—

	August 20.	August 21.	August 23.
Water	929.10	941.91	934.92
Solid residue	70.90	58.09	65.08
Urea	23.31	22.42	25.69
Uric acid	1.17	1.01	0.89
Lactic acid	1.55	1.01	1.35
Lactates	2.39	1.89	2.06
Extractive matter soluble in water	3.80	3.07	3.71
Extractive matter soluble in alcohol	17.84	13.78	15.77
Mucus	.12	.10	.10
Chlorides of sodium and ammonium	3.80	3.07	3.71
Sulphates	7.16	7.14	7.23
Phosphates of soda	3.54	3.68	3.74
Earthy phosphates	1.22	1.09	1.11

Analyses of the urine during a strictly non-nitrogenous diet of starch, sugar, gum, and almond oil. The urine, after this diet had been continued for twenty-four hours, became of a brownish-red colour, and a slightly acid reaction, which became alkaline in twenty-four to thirty-six hours.

The following analyses were made on the second and third days from the commencement of this course of diet, in the month of June:—

	(1)	(2)
Water	953.98	965.11
Solid constituents	46.02	34.89
Urea	18.92	11.08
Uric acid	.89	.54
Lactic acid and lactates	4.89	5.11
Extractive matter soluble in water	2.80	2.71
Extractive matter soluble in alcohol	8.32	8.78
Mucus	.11	.11
Chlorides of sodium and ammonium	2.74	1.14
Sulphates	3.25	2.98
Phosphate of soda	3.01	2.48
Earthy phosphates	1.00	.91

The following table gives the mean daily amount of the various solid constituents of the urine, excreted by the kidneys, during these different systems of diet:—

	Mixed diet.	Animal diet.	Vegetable diet.	Non-nitrog. diet.
	Grammes.			
Solid constituents	67.28	87.44	59.24	41.68
Urea	32.50	53.20	22.48	15.41
Uric acid	1.18	1.48	1.02	.73
Lactic acid and lactates	2.72	2.17	2.68	5.82
Extractive matters	10.49	5.20	16.50	11.85

The results of these experiments demonstrate the great influence which diet exerts upon the chemical and physical characters, and the relative amounts of the different constituents of the urine. The amount of solid matters excreted during the twenty-four hours, under a purely animal diet, was more than double that excreted during the same length of time under a non-nitrogenous diet. The amount of urea excreted under the former was more than treble that under the latter. The extractive matters were greatest under a vegetable, and least under an animal diet, while uric acid increased under the latter, and diminished under the former. If the vegetable diet had been persevered in long enough, in all probability, uric acid would have been replaced by hippuric acid, as happened in the case of a girl who fed entirely upon apples.

It is a well-known fact, that, in the urine of herbivorous animals deprived of all vegetable food, hippuric acid is replaced by uric acid. The important principles established by these experiments of Lehmann are confirmed by the differences which exist between the urine of the carnivora and herbivora. In animals living upon animal food, the solid constituents of the urine are in much larger proportion than in that of animals subsisting upon a vegetable diet. The urine of the lion, tiger, and panther contains, in the 1,000 parts, 154 of solid matter. The urine of the bat contains even a larger amount of solids. The urine of hard-working oxen and horses contains not more than from 70 to 100 parts of solid matter, whilst that of the pig contains not more than 20 parts in the same amount.

Lehmann likewise showed by his experiments that, during severe bodily exercise, urea and the lactates, sulphates, and phosphates are increased, whilst uric acid and the extractive matters are diminished.

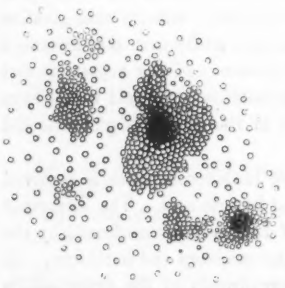
The following are the results obtained from a frequent examination of his urine during a pedestrian tour:—

	In 24 hours. *Grammes.*	In 1,000 parts.
Water	900.006	916.707
Solid constituents	82.594	83.293
Urea	45.314	45.697
Uric acid	.642	.647
Lactic acid	3.140	3.166
Extractive matters	8.455	8.526
Alkaline phosphates	4.598	4.636
Alkaline sulphates	15.047	15.174
Earthy phosphates	1.105	1.114

2. *Urine of Birds.*—In this class, the urine is characterized by being semi-solid, and consisting, in great measure, of the urate of ammonia, in the form of minute globules, which, when dry, resemble chalk. The following are several analyses which I made last summer.

a. Carnivorous Birds.—Qualitative Analysis of the Urine of the Osprey, or Fish-hawk (*Pandion Haliœtus*). The urine of this bird consists of two distinct parts; a white, chalk-like precipitate, and a light yellow, ropy, supernatant fluid, resembling much in its appearance albumen. That it does not contain albumen is proved by the fact that it is coagulated neither by heat nor nitric acid. Under a magnifying power of 210 diameters, the solid white portion consists of innumerable small globules of the urate of ammonia.

Fig. 18.



Appearance presented by the solid white portion of the urine of birds and reptiles, under a magnifying power of 210 diameters. To the naked eye, this resembles chalk; under the microscope, it consists of innumerable minute granules of the urate of ammonia.

Their appearance is represented in Fig. 18. When heated in water, this chalky mass was dissolved, and deposited again upon cooling. When heated to boiling, the ammonia was driven off as a gas, whilst the uric acid was deposited in the form of rhomboidal, lozenge-shaped, and hexagonal crystals. The presence of uric acid was further determined by treating the urine with nitric acid, applying heat, and then testing with aqua ammonia; the production of the characteristic purplish scarlet colour was the result. When treated with acetic or hydrochloric acids, numerous hexagonal, lozenge-shaped, and columnar crystals of uric acid, often conglomerated together in large numbers, were precipitated. When the fluid portion was

treated with aqua ammonia, multitudes of stellate crystals of the triple phosphate were deposited. When the fluid portion of the urine was simply spread upon a glass slide, and allowed slowly to evaporate, there appeared, under the microscope, crystals of the mixed phosphates and fixed alkaline salts, together with crosslets and cubical crystals of the chloride of sodium. We were unable, by a close examination of the urine with the microscope, after it had been treated with nitric acid, to detect any traces of the nitrate of urea.

The following are the principal constituents:—

Urate of ammonia, in large amount.	Phosphate of soda.
Phosphate of lime.	Chloride of sodium.
Phosphate of ammonia.	Purpurine.
Phosphate of magnesia.	No urea.

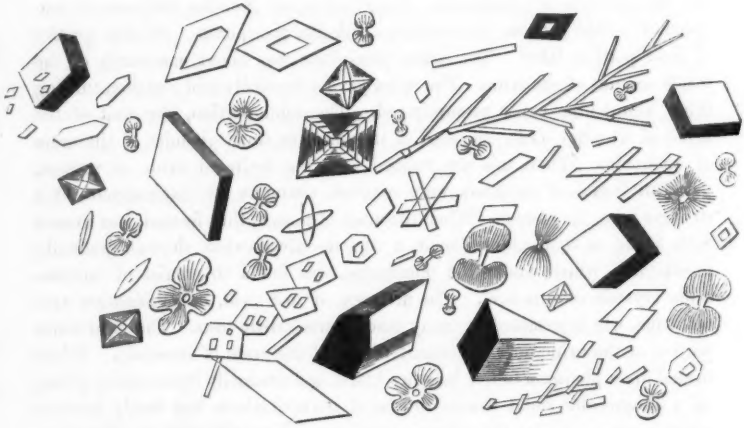
Qualitative Analysis of the Urine of the Small White Crane (*Ardea Candidissima.*)—The amount of urine secreted by this bird in twenty-four hours was three-fourths of a fluidounce. Like that of the *Pandion Haliætus*, it consisted of a fluid portion, and a white, chalk-like precipitate. Specific gravity of fluid portion 1060. The white precipitate was about one-fourth of the whole amount of the urine. The urine gave a decidedly acid reaction, turning thick litmus blue paper promptly red. The solid portion, like that of the urine of all other birds, consisted of innumerable small globules of the urate of ammonia. When this was rubbed with the hydrated oxide of potassa, powerful fumes of ammonia were evolved, owing to its displacement by a stronger base, the potassa. When the urate of potash, thus formed, was treated with acetic or hydrochloric acids, a copious silvery-white deposit gradually precipitated, which, under the microscope, was found to consist of innumerable crystals of uric acid. The presence of uric acid, in combination with ammonia, was determined by many other characteristic tests. The solid white portion of the urine does not consist wholly of the urate of ammonia. When heated upon platinum foil, it becomes black, and gradually burns away, giving off a disagreeable smell similar to that of burnt feathers, and finally leaves a white residue. Under the continued and more direct action of the blowpipe, a large portion of this fused into semi-transparent, vitrified beads. Portions of the ignited mass were not thus fused, but continued as a white powder, which gave an alkaline reaction with turmeric yellow, and dissolved in acids with effervescence. By various chemical tests, the residue was found to consist of oxalate of lime and phosphates of ammonia, lime, and magnesia. Penniform and stellate crystals of triple phosphate were precipitated from the fluid portion by aqua ammonia. When simply spread upon a glass slide, and allowed slowly to evaporate, prismatic crystals of triple phosphate, cubical crystals, and crosslets of chloride of sodium, and crystals of the other fixed alkaline and earthy salts, were seen under the microscope.

The following are the principal constituents of the urine of this bird:—

Urate of ammonia.
Phosphate of lime.
Phosphate of ammonia.
Phosphate of magnesia.
Phosphate of soda.
Chloride of sodium.
Oxalate of lime.

Qualitative Analysis of the Urine of a young, half-grown Green Heron (*Ardea Virescens*).—The urine of this bird consisted of two portions, and resembled, in all respects, that of other birds. When the transparent liquid portion was spread upon a glass slide, and allowed to evaporate slowly, a magnifying power of 210 diameters revealed numerous dumb-bell and octohedral crystals of the oxalate of lime, lozenge-shaped crystals of uric acid, prismatic crystals of triple phosphate, cubes of the chloride of sodium, and other crystalline forms of the fixed alkaline and earthy salts. The most important and striking of these crystals are represented in Fig. 19.

Fig. 19.



Dumb-bells and octohedra of oxalate of lime, lozenge-shaped crystals of uric acid, cubes and octohedra of chloride of sodium, and prismatic crystals of triple phosphate, deposited in the clear fluid portion of the urine of the green heron (*Ardea Virescens*). Magnified 210 diameters.

The solid, white portion of the urine, like that of the *Ardea Candidissima*, was found, under the blowpipe, to contain other ingredients besides the urate of ammonia.

The following are the principal constituents of the urine of the *Ardea Virescens*:—

Urate of ammonia.
Uric acid.
Urea.
Oxalate of lime.
Chloride of sodium.
Phosphate of soda.
Phosphate of lime.
Phosphate of magnesia.
Phosphate of ammonia.
Fibrinous casts.
Epithelial scales.

Epithelial scales and fibrinous casts were found in considerable numbers in the urine of this young bird. In this examination, we were surprised by the abundance of the crystals of the oxalate of lime.

The presence of oxalic acid in the excretions of the kidneys is always associated with some imperfection of digestion, and often of respiration. Young birds of this class are exceedingly voracious; they will gorge themselves with large and unnecessary quantities of food, and at the same time remain quiet and inactive. Here we have causes amply sufficient to account for the appearance of the oxalate of lime in the urine.

Urine of the Qua-bird, or American Night-heron (*Ardea Discors*).—The urine of this bird had the same appearance, and gave the same chemical reactions, and appeared to be constituted in all respects like that of other cranes.

In examining the urine of birds of this class, we are struck not only with the great amount of the urate of ammonia, but also with the large proportion of the earthy and fixed alkaline salts. Their presence in such abundance may readily be explained by considering the food of the birds, which consists of crabs, fiddlers, sea-conchs, and fish of all kinds, swallowed without any mastication. Although they eject the shells and bones from the stomach through the mouth, after the flesh has been removed by the gastric juice, still it is reasonable to suppose that a considerable amount of the fixed alkaline and earthy salts are dissolved from the shells, by the action of the gastric juice, simultaneously with the digestion of the flesh. The relatively large amount of the chloride of sodium must be derived from the salt water, a portion of which must be swallowed with every crustacean and fish.

The excretion of the kidneys of sea-birds is very abundant, and is the source of that valuable manure, guano. A faint idea of the immense quantities of this substance, deposited by myriads of sea-birds during many centuries, may be formed when we reflect that more than forty thousand tons have been imported to Great Britain and Ireland from the coasts of Peru, Bolivia, Chili, and Africa.

The following is an analysis, by Fourcroy and Vauquelin, of a sample of guano presented to them by Baron Von Humboldt:—

100 parts contained—

Urate of ammonia	9.0
Oxalate of ammonia	10.6
Oxalate of lime	7.0
Phosphate of ammonia	6.0
Phosphate of ammonia and magnesia	2.6
Sulphate of potash	5.5
Sulphate of soda	3.3
Sal ammoniac	4.2
Phosphate of lime	14.3
Clay and sand	4.7
Water and organic matters	32.3

The following is an analysis of Bolivian guano, by Andrew Ure:—
The soluble matter = 24 parts in 100, and consisted of—

1. Urea	5.00
2. Sulphate of potash	7.90
3. Chloride of sodium	5.00
4. Biphosphate of ammonia	5.50
5. Oxalate of ammonia	0.60

The insoluble matter = 69.5 parts, and consisted of—

1. Silica	2.25
2. Subphosphate of lime	9.00
3. Phosphate of magnesia and ammonia	1.25
4. Urate of ammonia	15.27
5. Undefined azotized organic matter, affording, with the 14 parts of uric acid, by ignition with hydrate of soda, 17.05 parts of ammonia	41.73

By comparing these analyses with those of the urine of birds, in its fresh state, we see that the urate of ammonia has almost entirely disappeared, whilst the fixed alkaline and earthy salts are relatively in much greater proportions. This is readily explained by the fact that uric acid is readily decomposed into ammonia and carbonic acid gas.

b. Urine of Granivorous and Frugivorous Birds.—In this class, the renal excretion is much less in quantity, and that of the large intestines much greater than that of carnivorous birds. In both classes, however, the urine appears to be similar in physical and chemical properties. The urine of several granivorous and frugivorous birds, I found, by microscopical and chemical examination, to consist chiefly of the urate of ammonia. Vauquelin and Fourcroy found that the urine of the ostrich contained uric acid to the amount of one-sixtieth of its weight. There were present, also, sulphates of potash and lime, chloride of ammonia, and a peculiar oily animal matter. According to J. Davy, the urine of the parrot is very similar to that of serpents.

3. *Urine of Reptiles.*—The urine of this class resembles that of birds, not only in appearance, but also in chemical constitution, being only a little more solid when first discharged. Here we have the urine of two wholly dissimilar classes alike in all respects. Birds have a high temperature, in many cases 10° higher than that of the mammalia. Their blood circulates with rapidity, and receives an abundant supply of oxygen. Their habits are those of activity.

Reptiles have no definite temperature; it corresponds with that of surrounding bodies. Respiration is imperfect, their lungs being, in most cases, simple sacs, around the walls of which ramify the bloodvessels. Their circulation is sluggish and imperfect, and their habits indolent and inactive.

Would any one suppose, *à priori*, that the secretions of these two classes would be similar? If the explanation given by Liebig be true, that the production of urate of ammonia in serpents is due to their imperfect respiration, and consequent incomplete oxygenation of the blood and tissues, why should

we have the same state of things existing in birds, who have oxygen supplied in large quantities, not only by their lungs, but also throughout the tissues, by means of their porous bones? It is useless to theorize upon this subject in the present state of our knowledge; we must first acquaint ourselves with the chemical constitution of the blood and tissues of cold-blooded animals, before we will be able to attempt an explanation of these apparently contradictory phenomena.

The urine of a land-tortoise (*Testudo Tabulata*), which had been starved for some months, was analyzed by Marchand. It resembled pus in appearance, and had a faintly acid reaction. He collected 1,337 grains, which gave the following analysis:—

		Or in 100 parts.
Water	1271.	950.64
Solid constituents	66.	49.36
Urea	8.5	6.40
Uric acid	23.0	17.25
Hippuric acid	none.	
Salts and indeterminate organic matter	34.5	25.70

The following is a qualitative analysis of the urine of the coachwhip snake (*Psammophis Flagelliformis*), which I made during the month of July:—

When the urine was allowed to stand for a short time, it formed a hard, white mass, which might have been readily mistaken for so much chalk. Under a magnifying power of 210 diameters, it was seen to consist of a conglomeration of innumerable globules of the urate of ammonia, resembling in all respects the solid portion of the urine of birds. Epithelial cells and fibrinous casts of the urinary tubes were also found in considerable numbers.

The following are the results of the microscopical and chemical examination of the urine of this snake:—

Urate of ammonia, in great abundance.
Phosphate of lime.
Phosphate of ammonia.
Phosphate of magnesia.
Phosphate of soda.
Chloride of sodium.
Epithelial cells.
Fibrinous casts of urinary tubes.

We were unable to detect the presence of urea. I have examined the urine of other snakes, as the pine-snake (*Pituophis Melanoleucus*) and the corn-snake (*Coluber Guttatus*), and found them to resemble that of the coachwhip in most respects, consisting mainly of the urate of ammonia. We have as yet been unable to detect urea.

In the kidneys of the glossy-black pine-snake (*Pituophis Melanoleucus*), several pyriform calculi were found imbedded in the substance of the kidney, extending from the anterior surface to the ureter. These calculi were found to

consist, in large measure, of the oxalate of lime. Other substances were also present, as urate of ammonia, and the phosphates of lime, ammonia, and magnesia.

The urine of the boa constrictor has been found, by Prout, to yield more than 90.$\frac{8}{10}$ of uric acid. The urine of the rattlesnake has been examined by Simon. The following is his analysis. 100 parts of the dried residue contained:—

Free uric acid, with a little fat and extractive matter . .	56.4
Urate of ammonia	31.1
Urate of soda, with some chloride of sodium	9.8
Urate of lime	1.4
Phosphate of lime	1.3

This analysis of Simon is calculated to mislead. The urine before analysis was thoroughly dry, all the water having been driven off, and with it much of the ammonia combined with uric acid. In no instance have I detected free uric acid, under the microscope, in the fresh urine of serpents; it exists always in combination with ammonia. The urine of the alligator and lizards consists principally of urate of ammonia. The fixed alkaline and earthy salts exist in considerable quantities in the urine of serpents, especially the salts of lime. Their presence in such abundance may be explained by a consideration of the food and habits of serpents. They are carnivorous, and swallow their prey, often of considerable size, without any mastication. In this manner, a large amount of the earthy salts are taken into the animal economy in the form of scales, bones, claws, teeth, &c. The bones frequently remain in the intestinal canal for a considerable length of time, and they must be acted upon more or less by the gastric juice. In the rectum of a large rattlesnake (*Crótalus Adamánteus*), six feet in length, whose intestinal canal was completely devoid of food, I discovered several balls of hair about two inches in length. When these were opened, they were found to consist of a mass of broken fragments of the bones of an animal, apparently of the size of a rabbit, so carefully wrapped up in the hair that not a single angular point protruded which might injure the intestinal canal. The bones were of a yellow colour, and appeared corroded in many places. The manner in which these bones were in the intestinal canal of the living snake, carefully wrapped up, is unknown.

The urine of the bullfrog (*Rana Taurina*), has been examined by J. Davy. It is a fluid having a specific gravity of 1003, and contains in solution urea, chloride of sodium, and a little phosphate of lime.

The urine of the *Bufo Fuscus* had a specific gravity of 1008, and contained chloride of sodium and phosphate of lime, together with a larger proportion of urea than the urine of the *Rana Taurina*.

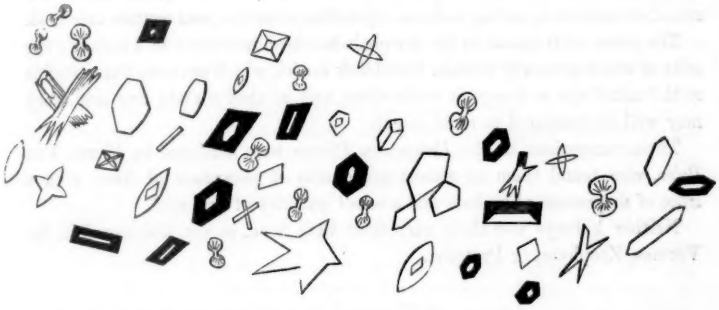
4. *Urine of Fish.*—We found it very difficult to obtain specimens of this for examination. The following is a qualitative analysis of the urine of the bassfish (*Corvina Ocellata*):—

Uric acid.
Oxalate of lime.
Phosphate of lime.
Phosphate of ammonia.
Phosphate of magnesia.
Phosphate of soda.
Chloride of sodium.

When the urine was spread upon a glass slide, and allowed slowly to evaporate under a magnifying power of 210 diameters, there appeared lozenge-shaped and hexagonal crystals of uric acid, dumb-bell, and octohedral crystals of oxalate of lime, and prismatic crystals of triple phosphate.

The appearance presented by the urine thus treated is represented in Fig. 20.

Fig. 20.



Crystals of uric acid, chloride of sodium, phosphate of soda, oxalate of lime, and triple phosphate, which made their appearance under a magnifying power of 210 diameters, in the urine of the bassfish (*Corvina Ocellata*), which was spread upon a glass slide, and allowed slowly to evaporate.

2. *Urine of the Invertebrata.*

Not much attention, as yet, has been paid to this subject. The following are the results of the investigations of Siebold and others.

The urine of the Insecta consists of a fluid containing numerous granular masses, which is poured from the kidneys into the digestive canal, and accumulates in the colon, or in its cœcal appendage, and is excreted with the feces separately or as a turbid liquid, of a colour varying with the species.

The Lepidoptera, when bursting from their pupæ, emit a considerable quantity of urine of a variable colour.

In the larva and pupa of Mymeleon it is of a rose colour, and gradually accumulates in large quantities in the digestive tube, which the perfect insect immediately discharges on leaving the pupa envelop, as a solid, elongate, ovoid body.

Red crystals, of a quadra-pyramidal form, are perceptible in the urine of many Insecta.

The urine of the Arachnoidæ is poured by the ureters into the digestive canal, and accumulates in the cloaca, and consists of a turbid, dirty-white liquid, holding in suspension innumerable dark molecules. When treated with nitric acid, heat, and ammonia, it gives the characteristic reactions of uric acid.

In the Crustacea, the urinary secretion has been observed only in the Myriapoda. This, like the urine of insects, contains uric acid.

In the Cephalopoda, the organs, for a long time known as the spongy bodies, have been demonstrated to be kidneys. The urine is secreted from their external surface, and is of a dirty-yellow colour, and escapes immediately into the peritoneal cavities, and thence is discharged externally through their orifices.

Not unfrequently reddish crystals, formed in the urine, completely invest the kidneys, giving to them their peculiar colour.

A chemical analysis of Harless shows that they contain uric acid.

The cells of the kidneys of the Cephalophora contain a transparent liquid, in which float obscure nuclei, of a brown or violet colour. These nuclei are round or embossed, having a dense crystalline structure, and contain uric acid.

The gland of Boganus in the Acephala has been proved to be a kidney; the cells of which generally contain blue-black nuclei, which are sometimes visible to the naked eye as inorganic concretions, and, as they contain uric acid, they may well be compared to renal calculi.

These concretions in the *Petunculus Pilosus* were analyzed by Herrn Von Babo, who found them to consist principally of phosphate of lime, with a trace of magnesian phosphate and a small quantity of uric acid.

Neither kidneys nor their excretions have been, as yet, discovered in the Vermes, Zoophyta, or Protozoa.

ART. II.—*Account of the Asiatic Cholera, as it prevailed in Columbia, Lancaster County, Pa., in the Autumn of* 1854. By T. HEBER JACKSON, M. D. (With a plan of the town.)

IT is only by carefully observing and recording the conditions under which epidemic cholera prevails, that we can hope to discover its cause, and the laws by which it is governed. With the hope of contributing some materials for this purpose, we have drawn up the following report:—

It is no easy matter to follow distinctly the progress of an epidemic when it prevails extensively in a large and populous city; but in a small town, its origin and progress can be more readily traced, and we shall endeavour to do this in regard to the disease as it occurred in the town of Columbia, Lancaster County, Pa., during the month of September, 1854.

It will be useful to consider, briefly at least, the situation and aspect of Columbia, and all such conditions as might reasonably be supposed to have exerted an influence upon the development and propagation of the epidemic in that town. To this end, the reporter avails himself of a communication from Dr. A. CLARKSON SMITH, of Columbia.

"Columbia is situated on the north bank of the Susquehanna, forty miles above the Chesapeake Bay, and is the terminus of two canals and three rail-

roads. The town lies on a gentle slope inclining to the northwest. Just above the town, high, bluff hills jut out into the channel of the river; while below, another ridge, running parallel with the one above, separates it from the town of Washington. North and northeast there is a valley of the most fertile land in the country. Our population is made up of representatives of almost all nations. The German and Irish, however, compose the principal portion of the foreign population. Of the latter, there are many. The railroads, canals, coal-yards, and commission warehouses give employment to so many persons of this kind, that, in their vicinity, they generally compose the bulk of the population. As a general rule, these persons drink freely of alcoholic stimulants."

The river flows with a gentle current, and for a considerable distance above and below the town is quite shallow. The surface is studded with innumerable islets covered with a rich vegetation, which adds much to the beauty of the scene, though not, perhaps, to the salubrity of the locality. Above Washington, and a short distance below the town, a dam has been constructed for the purpose of backing the water into the canal. This structure is provided with an outlet or sluice. Above the dam—in that section of the town bordering upon the river, and west of north—is the "basin," which empties its contents, by means of the outlet-lock, into the river. Between the dam and the outlet-lock are the "reservoirs" from whence the hydrants are supplied. The condition of the river, the basin, and hydrant-water will engage our attention when we come to investigate the possible causes of the appearance of cholera in Columbia. "For several weeks previous to the appearance of cholera, remittent fever had been unusually prevalent. It began generally with bilious diarrhœa, or when this looseness of the bowels was not found, there was an extraordinary susceptibility to the action of cathartic medicine. The great prevalence of remittent and intermittent fever had left many persons debilitated, and in a condition the most susceptible to the action of any epidemic influence which might chance to come upon them. As a *general rule,* persons thus debilitated were *first attacked* by cholera." Some weeks previous to the appearance of cholera as an epidemic in Columbia, the inmates of a house on Front Street, not far from the place of its reappearance, were attacked by the disease in its most fatal form, and several fell victims to its violence. This building was destroyed by order of the town authorities, and no new cases occurred until Wednesday, the 6th of September. "At this time," writes Dr. Smith, "the emigrant train, west, left at our depot two German emigrants, sick with cholera. One of them died during the night; the other, a boy, seemed to recover from it, but afterwards died with what, I have no doubt, was ship fever; the vessel in which they crossed the Atlantic having had that disease, with cholera, on board. On Friday evening two cases occurred among our own citizens, both of whom were unhealthy men; one of these having suffered with diarrhœa for two weeks previously. At midnight, it made its appearance in almost every section of the town, and at daylight there were *thirty cases,* all of which proved fatal during Saturday. Of those who fell under my care, I recognized many whom I had noticed in the room with the German emigrants during their illness. Most, if not all of these persons had diarrhœa the day previous, but as there was nothing to alarm them, paid no attention to it until too late. Most of them were sinking into collapse when first seen by the physicians."

The class of the community chiefly affected by the disease was composed of German labourers. But on this fatal night *all* portions of the town, *all* classes of persons were compelled impartially to contribute victims to the

merciless pestilence. By referring to the accompanying plan of the town, this will clearly appear. A panic now seized upon the citizens, and many of those whose means enabled them to leave, fled from the devoted town. By Monday, it is calculated that more than half of a population, numbering some 5,000, had left, and numerous persons left daily, until the week was far advanced.[1] It is necessary to mention this, lest the erroneous conclusion might be drawn, that the working class was much more obnoxious to the disease than they really were. It is fair to presume, from the history of the epidemic during Friday night and Saturday, that, had all the citizens remained, no distinction of *class* would have availed as a protection, but all would have suffered alike, in proportion to their numbers. Upon those whose health was vitiated by intemperance, and the many privations attendant upon poverty, the arrows of pestilence might be expected to fall with fatal effect; but it did not appear, on Friday night and the succeeding day, that these conditions served to invite or determine the direction of their flight.

It is proper, now, to investigate the cause of the appearance of cholera in Columbia. This town is peculiarly liable to those morbific influences which are supposed to generate malarious diseases. Accordingly, we find the physicians of the place report the almost constant prevalence amongst them of intermittent and remittent fever, and, did the statement need confirmation, it is to be found in the appearance of the inhabitants. An unusual continuance of dry and very warm weather had lessened the volume of the river to a degree unknown for years; had exposed and promoted the decomposition of the vegetable matter on its banks and islets, and diminished the rapidity of its current. And, in addition, it is stated in a Columbia paper that "the canal had been made the receptacle for animals dying in the cars while being transported to the eastern markets, thrown in during the night, and washed to the shores to putrefy in the blazing sun." The sluice being closed, by direction of the Tide-Water Canal Company, the escape of the double products of animal and vegetable decomposition was most effectually prevented. Upon the sluggish waters of the river, therefore, and upon its banks and islets were to be found, in even greater than usual abundance, the noxious properties, which, co-operating with a uniformly high temperature, are so generally supposed to generate malarious diseases. And there were very many sick with bilious fever, intermittent and remittent; and there were likewise many cases of bilious dysentery and diarrhœa. This might have been anticipated. The history of this year, so far, differed in no essential respect from that of former years. For the same causes, real or apparent only, had always been present at this season of the year, in a state of greater or less intensity, and like diseases, as effects, attended. In degree, as in kind, effect answered to cause; for the condition of the river and its precincts being such as to afford more than the usual amount of miasmata—"for several weeks previous to the appearance of cholera, remittent fever had been *unusually* prevalent." Cases of the diseases mentioned, continued to occur up to

[1] It will not be amiss to mention that many generous men having removed their families, returned that they might aid the sick.

Friday, when the cholera broke out; they *then* disappeared, and reappeared only when the cholera epidemic was abating.

In former years, the diseases peculiar to the several seasons were impressed with the leading characteristics of the malarious fevers; this was true in regard to pneumonia, functional derangements, and organic lesions of the stomach and bowels, and also entero-mesenteric fever. But now, cholera has entirely superseded the malarious fevers, and is, by no means, impressed with their characteristics. The two diseases are essentially different, and will hold no intercommunication. Are they, then, effects of the same cause? The hygrometric and thermometric condition of the atmosphere continued the same; the state of the river, of its putrid burden and noisome shores, was unaltered—at least there was no apparent change; and yet the usual and concomitant fevers have suddenly disappeared, and, in their stead, is this strange pestilence! Some were satisfied to assign miasmata as a sufficient cause. Strength, though not actual confirmation, will be lent to this hypothesis by a knowledge of the following curious phenomenon: On Friday, about noon, a southeast wind began to blow—of course, blowing over the river directly into the town. On the afternoon of this day, the first case of cholera occurred. This wind prevailed steadily until Sunday morning, when it changed to the northwest, blowing in a direction directly from the town to the river. A very sensible diminution in the number of new cases was observed at once, and when the wind veered to the original quarter (southeast), as sensible an increase was perceived. This curious coincidence (?) was noticed *at the time* by several. Indeed, it was a matter of general remark, and the "direction of the wind" was being constantly questioned with marks of deep interest and solicitude. But remarkable as this may appear, before it is determined that emanations from the river, wafted into the town by this southeast wind, were productive of the cholera, it will be worth while to remember that during a long series of years Columbia had been exposed to precisely the same influences, the same combination of circumstances, and yet remained happily free from cholera. It is not denied that the condition of the river air, probably impure, may have afforded a suitable nidus for the disease, and that the wind, by wafting the exhalations—the blastema, so to speak—into the town, may have thus exerted a powerful agency in the advancement of the epidemic. Moreover, if the river and its shores are to be accused of having generated the cholera poison, why and how did the people of Wrightsville, on the opposite bank from Columbia, escape? especially when, as on Sunday, the strong northerly wind was blowing? And yet escape they did, without a single case. In addition to this, it is to be observed that Washington, on the *same* side of the river with Columbia, and distant only one or two miles, also escaped. When the cholera had disappeared from Columbia, a few cases were reported in Washington.

It may be said that Washington was below, and separated from the area of the peculiar miasmata, by the dam. But few would care to repose upon such a protection. We have already described the position of the basin and canal;

by some it was contended that their impure waters mingling with the river water, and being then pumped up into the reservoirs, and used by the inhabitants for drinking, and for culinary purposes, were alone concerned in generating the epidemic. But it was well known to all the physicians that very many who confined themselves, from habit, to the use of pump-water, were attacked with fatal effect. This is surely a sufficient refutation. Perhaps the most popular explanation was that which attributed the appearance of the malady to contagion. There is much to be said both for and against this hypothesis. In favor of the affirmative it may be urged that the appearance of the epidemic was subsequent to the arrival of the two emigrants, one of whom died of the disease; that many of those who were in attendance upon these aliens were amongst the first to be attacked; and, lastly, that many cases can be instanced, in which, owing to peculiar circumstances, the disease *could* have been contracted in no other conceivable way than by contagion.

The first proposition, that of the sequence of the local epidemic upon the arrival of the imported cases, is true enough. Unless taken, however, in conjunction with, and supported by other and more weighty facts, it is really worth nothing; for it is obviously unfair to conclude that, because the cholera appeared *after*, it must have depended for its existence upon the arrival of the emigrants. But this argument for contagion acquires importance when taken in connection with the next fact, viz: that many of those attacked had communicated directly with the sick emigrants. "Of those who fell under my care," says Dr. Smith, "I recognized many whom I had noticed in the room with the German emigrants." But "at *midnight* the cholera made its appearance in *almost every section* of the town, and by *daylight* there were *thirty* cases, all of which proved fatal during the same day." Now, amongst these *thirty cases*, what more likely than that some, or even "many of those seen in the room with the German emigrants," should have been found? Doubtless, there were *many more* drawn by curiosity, or more laudable motives, about the emigrants who were *not* attacked. But, above all, it is worthy of notice, that cases of Asiatic cholera had occurred in Columbia previous to the arrival of the emigrants; and if contagious *now*, the disease did not appear to be so at that time.

After the arrival of the reporter in Columbia, on Sunday the 10th, he neither heard of, nor was himself conversant with any case that could be fairly traced to contagion. Of all the physicians (except the much to be lamented Dr. Cochran, who was seized on the first, and died on the second day of the epidemic), officers of the sanitary committee, and nurses in attendance upon cholera patients in private practice, or at the Town Hall, but *one*, a nurse, was attacked! And yet all these were most zealous and constant in attendance upon the sick. The emigrant who died of cholera, died on the night of the day on which he reached Columbia; no one of the citizens was attacked before Friday evening. Is this, then, to be considered the duration of the incubative stage of cholera, if contracted by contagion? Perhaps the period of incubation varies—for, between midnight and daylight,

there were thirty cases. But did the disease spread as those known to depend upon contagion are propagated, viz: from one to another, at appreciable intervals of time? No: "at midnight it made its appearance in almost every section of the town, and by morning there were thirty cases!"

Surely, this malignant disease which thus, in a few hours, invaded the whole town, could not have been produced by contagion, which requires either actual contact, or at least the reception into the body of some emanation from an individual affected with a similar disease.

But all of the thirty cases had not been in contact with the emigrants, and it is probable that not more than two or three had been so actively engaged about the persons of the sick, as were the numerous physicians and nurses, and yet but one of the latter died! Moreover, it is necessary that these poisonous emanations from the person of one labouring under a contagious disease, should not only be inhaled, or otherwise effect an entrance into the body, but that they should likewise undergo some change, in which the body participates before it can become a new focus of contagion. This change requires time, which varies much according to the susceptibility of the individual. But, on Friday night, thirty were seized almost simultaneously.

Contagious diseases do not seize upon great numbers at once, but progress from case to case. Therefore, it does not appear that the disease was simply contagious, nor that the cause of its extensive spread was of an epidemico-contagious character. Doubtless, there was some modification of the atmosphere favourable to the extension of the disease *in the town;* but this appeared to be connected with some *local* condition, for those living in the country and near to the town, escaped altogether. In company with Dr. Smith, the reporter visited many individuals in the country adjacent to, and within a mile or two of Columbia; these were affected with various ailments, and one, a gentleman advanced in years, and liable to derangement of the bowels, appeared to have cholerine; but he readily recovered under the use of simple measures. The reporter believes this to have been the only case of the kind in the country. Communication with the country, during the first three or four days of the epidemic, was indeed very limited.

Still, physicians from the town visited their patients in the country, living near to Columbia, and yet the disease, if portable, was not conveyed to any one.

Hence, it would appear that the cause of the appearance and spread of malignant cholera in Columbia, was manifestly connected with the air and locality; that it was eminently endemico-epidemic.

Before quitting this subject, it will be proper to give some individual cases which would appear to favour the doctrine of the contagiousness of Asiatic cholera: "A gentleman from Bainbridge visited this place during the prevalence of cholera, returned home, a distance of seventeen miles, took the disease and died. His family fled, and he was left in charge of a friend who also took it, as did another who assisted in his burial. Neither of these persons had been to Columbia, nor at any place where they could have contracted it save from his person." Isolated cases of this kind by no means prove

that cholera is contagious, though they serve to render it probable. They should not, however, be regarded apart, but in connection with the host of facts rising up on every side to prove the opposite, and amongst which so vast a multitude, a few eccentric phenomena might well be supposed the result of accident. Hundreds left Columbia during the height of the epidemic some of these died of the disease away from home; but, so far as the reporter could learn, in only one or two cases did there appear to be a communication of the disease to others.

To attempt the description of a disease which has been so frequently and faithfully portrayed by able writers, that one conversant with their writings cannot fail to recognize it, would be superfluous. It will be sufficient to mention some modifications under which the disease appeared in Columbia. Premonitory symptoms were present in nearly all the cases, for periods of time varying from several days to a few hours. The advent of the disease found many labouring under bilious diarrhœa; many also, weakened by recent attacks of remittent fever; "and, as a *general rule*, persons thus debilitated were *first attacked* by cholera." The characteristic discharges from stomach and bowels were present in all the cases; in some, profuse; in most, however, remarkably moderate, and yielding with unusual facility to the agents employed to arrest them. But the welfare of the patient oftentimes did not appear to be promoted by their suspension, nor could a favourable prognosis be founded upon the circumstance of their being small in quantity. While thus mild in the particulars specified, the case frequently presented all the other worst symptoms of the disease, viz: a rapidly failing pulse; hurried respiration; anxious expression of countenance; altered tone of voice; increasing coldness, and diminishing resiliency of skin; great restlessness, and violent cramps. The nervous symptoms were the most prominent. Hence, the poison may be supposed to have acted directly upon the nervous system, without its usual irritant action on the mucous membrane of the alimentary canal; or else, that the effusions really took place into the bowels, but were retained. Opportunity to determine the last by actual inspection did not offer; but the physical signs failed to indicate the presence of a large amount of fluid in the bowels. The altered voice, when present, was regarded as most unfavourable. The cramps were chiefly confined to the muscles of the lower extremities, but in some extended to the dorsal, abdominal, and in a few to the brachial muscles. These spasms induced extreme suffering. When reaction succeeded to the stage of collapse, it was usually attended with the greatest danger, from the liability to cerebral congestion; this was more especially the case with children, of whom many were attacked.

In the treatment of this disease—many agents, of the most diverse characters—many methods, some with, some without a reason, or a good one—were tried. But, from the results, could be gleaned no argument in favour of the empirical practice. Indeed, the success of rational treatment was most gratifying—inasmuch as many were saved, who, for the lack of it, could not have recovered. In the stage of collapse, even, means were sometimes found to

restore to the blood its lost elements, and cause the heart again to propel its life-giving contents to the exhausted capillaries. Such general measures as experience and observation had recommended, were adopted by the Sanitary Committee; these it is unnecessary to enumerate. By the delegates from the College of Physicians of Philadelphia, the resident and visiting physicians, a series of instructions were drawn up, and, by the Sanitary Committee, printed for general distribution. A notable improvement in the health of the town followed their publication, from which, perhaps, a faithful compliance with the instructions may be inferred. (?)

It would be alike tedious and unprofitable to mention all the means and drugs employed in the treatment of the disease.

In view of the great discrepancies that exist as to the pathology and nature of the disease, that treatment was the safest, and no less successful, which was based upon indications drawn from the symptoms. If the physician was so fortunate as to see a case in its incipiency, he seldom experienced difficulty in arresting its progress. The diarrhœa, unless clearly to be traced to the presence of irritating ingesta, was met at once, and successfully with astringents, the particular article of the class used being determined by the preferences of the physician.

Opium, or some one of its preparations, was almost always used in conjunction with a mineral or vegetable astringent. Large opiate enemata were used with the greatest advantage when everything else had failed. It may be observed now that throughout the treatment of the disease, perfect rest and quietude in bed were deemed essential to success.

In large or small doses, some preparation of mercury was very generally employed, and that throughout the attack. In the first, second, and third stages, to restore the secretions, especially that of the liver; in the fourth stage, that of reaction, as a sedative, and for its general alterative action.

Unfortunately, a large number of patients were, when first seen by the physician, already in a collapsed state, and the end, not far distant, was but too frequently fatal. Nevertheless, some even of the worst of these recovered under judicious management. The great object in this stage was to prevent the further loss of the serum of the blood, to allay nervous irritability, and support the system until it could react. To accomplish the first indication, the most powerful astringents were used, preferably by injection; while, to supply the waste already sustained, ice-water in small quantities at a time was freely given, a salt of soda in small doses being added from time to time. Very large doses of opium were given in this stage, without inducing any impression whatever; when carried still further, a comatose condition was induced, which would cease in a short time, if the narcotic was discontinued.

Though the application of friction and warmth to the surface was markedly disagreeable to some, many yet seemed to derive relief from their use. These applications oftentimes seemed to lessen nervous excitement, to have a quieting, soothing effect upon the patient; and, certainly, were the most useful of

all the agents employed for the relief of the cramps. These spasms were most rebellious in spite of all that could be done to control them. Bandaging failed to afford more than very temporary relief. Beef-tea, animal broths, salted to the taste of the patient, and likewise various stimulants were given to support the strength and equalize the circulation, so far as the condition of the blood would permit.

Perhaps sufficient caution is not used in the exhibition of stimulants. In some cases—those in which the blood, robbed of its serum, has become of the consistence of a syrup—it is vain to think of deriving advantage from stimulants. If the stomach will tolerate the intrusion of dram after dram of alcohol, and quickly recurring doses of ammonia, what good will their presence in the stomach effect? Can they impart normal fluidity to the blood, or enlarge the caliber of the arterioles and venules. Even if they could greatly increase the force and frequency of the heart's action, which, fortunately, they cannot, would it not be dangerous to employ them? For a fluid of the consistence of that now found in the heart and large bloodvessels, cannot be forced through the capillary system, without rupture of these delicate vessels and consequent extravasation. Moreover, the blood in this condition is not fitted to fulfil the functions for which it was designed; therefore, care should first be taken to repair it, and it will then circulate readily enough. But, in those cases in which the discharges have not been very large, internal and external stimulants may be used with advantage. But here something is wanted which will produce a sudden and powerful impression upon the whole system; and, to this end, the reporter believes nothing will be found to surpass the hot salt-bath in efficacy. In conjunction with Dr. Smith, the reporter made frequent trial of this agent, and the result was highly pleasing to both. It was not until the epidemic had continued some days, that this agent was had recourse to, otherwise it would have been employed much more frequently.

"In the stage of collapse," writes Dr. Smith, "the hot salt-bath was more efficacious than anything I am aware of. Seven apparently desperate cases recovered after being immersed in it. One of these had been in collapse full thirty hours." The water should be raised to a temperature the highest compatible with safety. Prior to the immersion, the patient should take some stimulus, whatever has proved most grateful to his stomach. On removal from the bath it would be well to repeat the excitant, while, at the same time, friction with coarse towels should be practised, and the patient enveloped in blankets. Heated bricks, wrapped in some coarse material, which has been steeped in water, should be disposed along the person of the patient beneath the bed-clothes. For adults, it is necessary that the temperature of the bath should exceed 98° Fahr. Children, having a delicate cuticle, and being much more impressible, do not need so high a temperature. The patient, subjected to this treatment, more especially if a child, should be closely watched, lest reaction run too high; or, having lasted a short time, should begin to be followed by depression. This did not happen in any of the cases seen by the

reporter, but it is well enough to be on our guard against the possibility of its occurrence.

It is well known that patients in the third stage of cholera, though with a cold surface, suffer extremely from hot, or even warm applications; hence the use of the hot bath is not unattended with pain. This, though to be regretted, should not deter us from employing it when it is indicated. In children, when the disease could not be cut short, it was a comparatively easy matter to conduct them through the first three stages of cholera; but the fourth was fraught with the greatest danger. "The difficulty in managing children in this disease was not in checking the vomiting and purging, nor even in rescuing them from collapse (a simple astringent mixture, and placing them in the hot bath, being mostly sufficient to accomplish these purposes), but in preventing an attack of congestion of the brain, which often proved fatal." By the early employment of topical depletives, of derivatives, and the alterative action of mercury and ipecacuanha, this morbid condition was sometimes overcome. The reporter believes general bloodletting was practised in only one case, and then at so late a stage of the attack that but little hope could be reposed in any remedial measure whatsoever. When the cholera made its appearance in Washington, Dr. Smith made use of venesection, and the result is given below in a quotation from his letter to the reporter:—

"In the second or serous stage, I know of no remedy equal to the lancet; and I am only sorry I did not use it when the epidemic first made its appearance, so as thoroughly to have tested its efficacy. It was not until after the disease ceased to prevail as an epidemic that I determined to try its effects, and in five cases in which bleeding was practised all rapidly recovered. I was called to see a young married lady, of full habit, who had had diarrhœa for forty-eight hours, and the morning I visited her had the characteristic serous discharges from the stomach and bowels, attended with most distressing cramps in the extremities. Altogether, it was as severe a case of the second stage of cholera as I saw during the epidemic; and feeling assured she would die under the ordinary treatment, I determined to try the effect of bleeding. The blood was drawn in a full stream from a large orifice, and when she had lost ℥xvj, she complained of feeling faint. The vein was closed, and gr. ¼ morphiæ sulph. given. After this, she had not another cramp; the purging and vomiting ceased, and with the above-mentioned combination of soda and water, she rapidly recovered. Subsequently, I bled four others with like results."

Should this report appear meagre and too general in its character, the fact that no notes were taken by any of the physicians will sufficiently account for it, as well as for deficiencies and errors, should any appear.

The reporter feels it is but just he should state that he spared himself no trouble in the endeavour to obtain accurate information.

On the accompanying plan of the town, has been indicated by numerals, the position of the residences of fourteen of those who were seized on the evening and night of the 8th of September. The numerals indicate also the order of succession in which the cases occurred, or rather were seen by the physicians. They took place in very rapid succession, almost simultaneously, indeed. By referring to the table, a synopsis of each case will be found.

No.	NAME.	RESIDENCE.	TIME OF ATTACK.	TIME OF DEMISE.
1	Francis Bradley,	Front Street	Friday afternoon	Died in a few hours.
2	E. A. Howard,	Front Street	Friday evening	Saturday.
3	Mrs. Wm. Hippey,	Cherry, between 4th and 5th Streets	Friday night, visited No. 1, on Friday afternoon	Saturday at 8 A. M.
4	Dr. R. E. Cochran,	Walnut Street	Saturday morning early, had diarrhœa two days previously	Saturday, at 12 M.
5	Margaret L. Hagan,	Walnut Street	Friday night	Saturday, at 8 A. M.
6	James Keely,	Harkin's Tavern	Friday night	Saturday, at 10 A. M.
7	Mrs. S. Felix,	Fourth Street	Friday night	Saturday, at 1 P. M.
8	Mrs. S. Atkins,	Union Street	Friday night	Saturday, at 12 M.
9	Mrs. Richards,	Third Street	Friday night	Saturday.
10	Robert Spratts,	Fourth Street	Friday night	Saturday morning.
11	Peter Remler,	Perry Street between 2d and 3d	Friday night	Saturday night.
12	John Gheen,	Perry Street between 3d and 4th	Friday night	Saturday.
13	Lorenzo Krab,	Third Street between Union and Perry	Saturday	Sunday.
14	J. J. Strickler,	Herr's Hotel	Saturday morning	Saturday.
15	H. H. Lichty,	Locust Street	Friday night	Saturday.
16	Samuel Hinkle,	Union Street	Saturday morning	Saturday.
17	Mrs. S. Lysle,	Lauren's Street	Saturday morning	Saturday.
18	John Gilbert,	Perry Street	Saturday morning	Saturday.
19	Miss Ann Harnly,	Locust Street	Saturday morning	Saturday.
20	Mrs. C. David,	Union Street	Saturday	Saturday.
21	John Boyd,	Locust Street	Saturday	Saturday.
22	Chas. Benner,	Union Street	Saturday	Saturday.
23	—— Weaver,	Minnich's Tavern	Saturday	Saturday.
24	Chas. Jackson (col'd),		Saturday	Saturday.
25	Webster Fox (col'd),		Saturday	Saturday.
26	Mathias Neidinger,	Union Street	Saturday	Saturday.
27	German, boy,	Name and residence unknown	Saturday	Saturday.

The reporter has not marked on the plan the position of the residences of the last thirteen cases given in the table; he could only ascertain the street in which each lived.

The total number of deaths from cholera, as reported, was 127.

PLAN

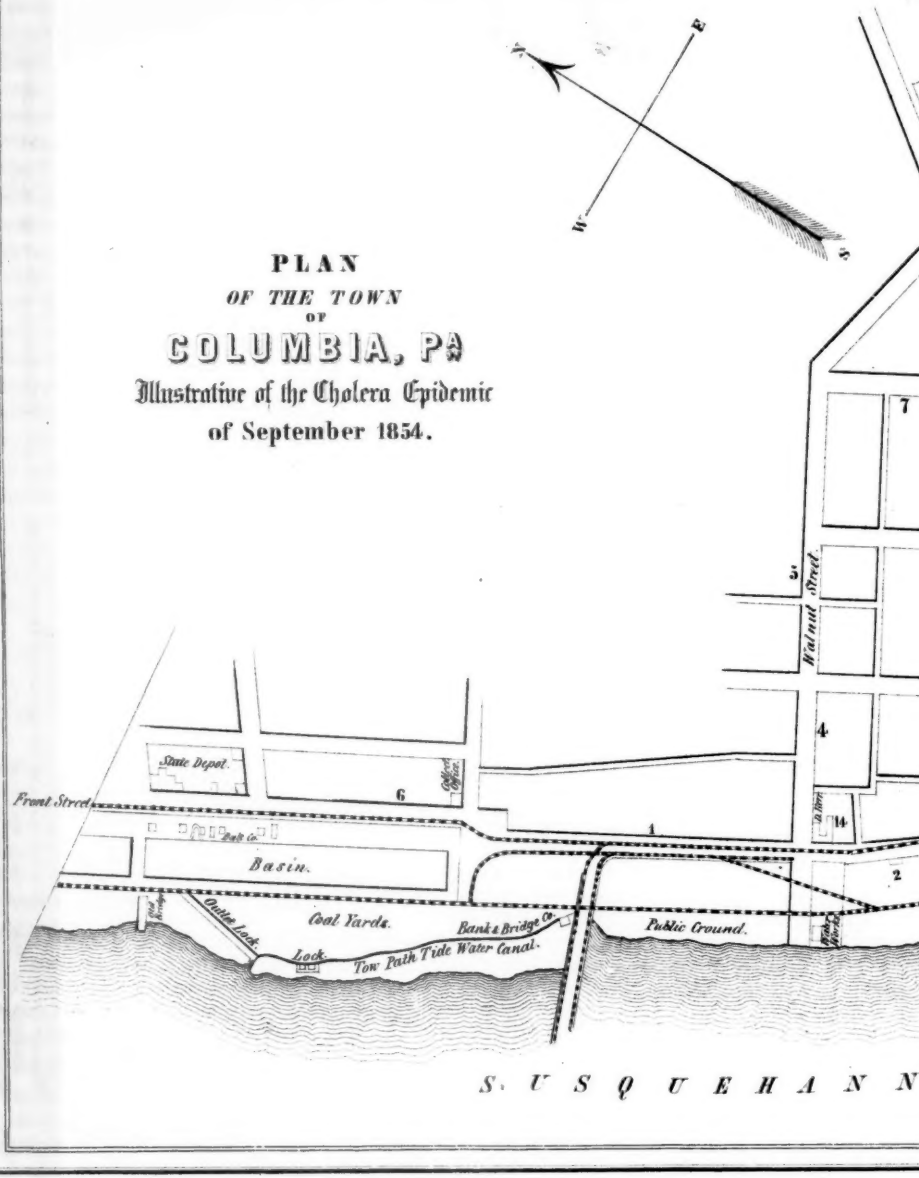
OF THE TOWN

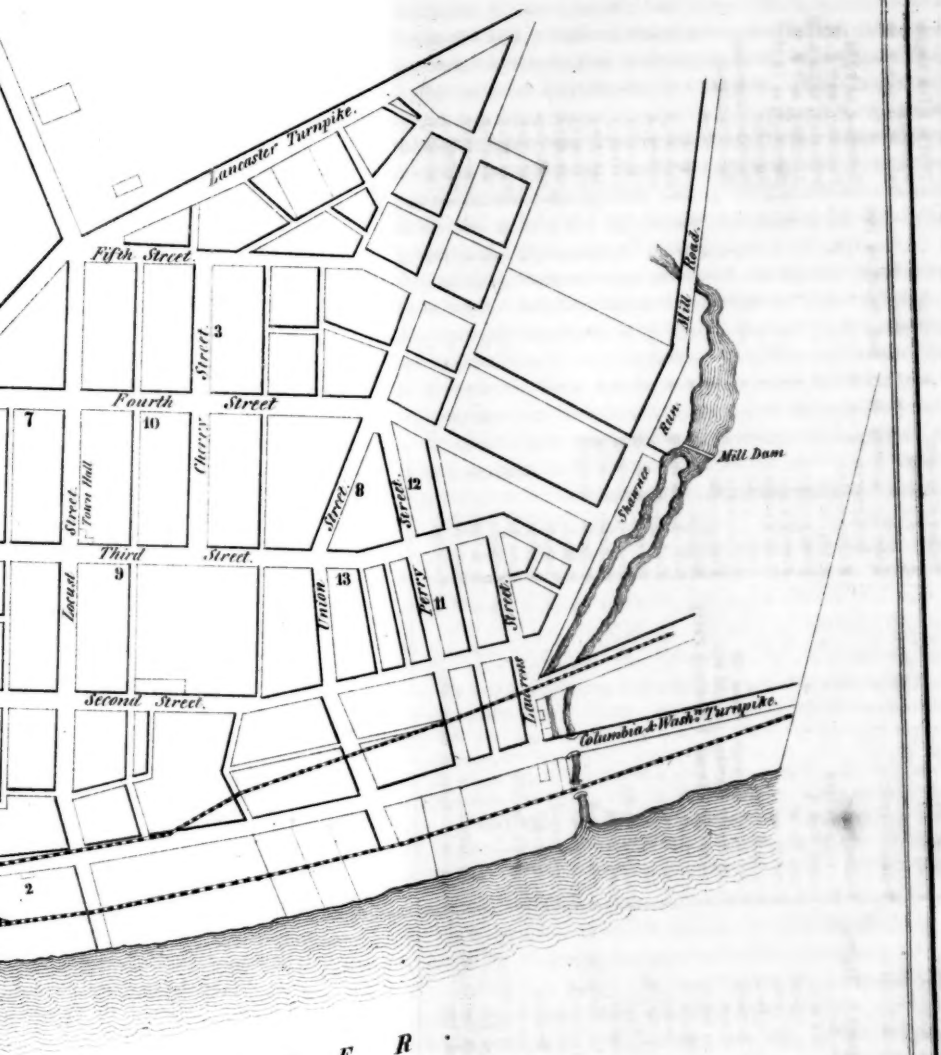
OF

COLUMBIA, PA

Illustrative of the Cholera Epidemic

of September 1854.





N A R I V E R .

Th. Sinclair's lith. Phil.a

Art. III.—*Extracts from the Records of the Boston Society for Medical Improvement.* By Wm. W. Morland, M. D., Secretary.

November 13, 1854. *Unusual Appearances in the Larynx in two Cases of Typhoid Fever.*—Dr. J. B. S. Jackson showed the specimens, taken from hospital patients. 1. Erosion of the epiglottis, the whole top of which has been entirely removed. The cartilage stands up, quite denuded, and of a dull-yellowish bronze colour, nearly or quite a line above the termination of the mucous membrane, which last appears perfectly healthy. Contrast between the apparently dead cartilage and that below the line of erosion was striking. The larynx was otherwise healthy; but in the pharynx, and near to the glottis, was an irregular, thin, quite defined ulcer, through the mucous membrane, and about two-thirds of an inch in extent. The patient, a man, æt. 23, had been sick about four weeks; local symptoms mainly pulmonary, and nothing was known of any affection of the throat. Congestion of the lungs was found, with the usual affection of Peyer's glands.

2. Something like a gangrene of the mucous membrane upon the inside of the larynx. The appearance is confined to the left side, and is nearly limited in extent by the thyroid cartilage. The affected tissue retains its polish upon the surface, and its consistence and thickness are not perceptibly changed; but there is a tawny opaque hue, which is not merely strongly marked, but perfectly defined, and showing even some indications of a line of separation. There is no effusion into the cellular tissue beneath this patch, and little or no redness of the mucous surface about it. From the vocal cord to the top of the larynx there are the appearances of inflammation; but still, they are confined to the left side, the right being quite healthy. The parts are much swollen, flabby to the feel, and upon the rima glottidis is an ill-defined, superficial, tawny discoloration, as if threatening to slough. About this discoloration are some small, vividly red spots; but otherwise there is no redness of the mucous membrane, nor any other unusual appearance. Upon incision, there is very little effusion of serum into the swollen parts, and no pus.

The patient was a man, 32 years of age, and had been ill five weeks, but without any especial local symptoms. The voice was rough for the last two weeks, during which time he was in the hospital; but disease of the larynx had never been suspected. In the intestines were the usual appearances of Peyer's glands.

Death from Hæmoptysis.—Dr. Charles E. Ware reported the case. The subject was a young man, 35 years of age, who had his first attack of hemorrhage in the autumn of 1852. Dr. W. was called to him in July, 1853. He had at that time an attack of hæmoptysis; quantity about half a pint. He had no cough, and had had none. He had not recently lost flesh nor strength, but rather gained. He had distinct crepitus, dulness on percussion, and resonance of the voice under right clavicle. He kept about his business, and the hæmoptysis was not renewed at the time. Dr. Ware was called to him again, November 6, 1854. He had enjoyed nearly his usual health, which was not very reliable, until the day previous, when, walking in the street in Lowell, he was seized with quite a copious hæmoptysis. He got into a carriage, drove to the cars, and came to Boston. The hemorrhage had ceased, and he arrived in his usual health. A physical examination of the chest was postponed until

the excitement attending the accident should be past. He remained quiet until the 10th, when, feeling well enough, he thought, to take a walk, he went out into the street. He had only walked about a quarter of a mile, when he was attacked by hemorrhage, and died immediately, apparently from suffocation; for the loss of blood was not more than half a pint. No examination was obtained.

Dr. PARKMAN mentioned a case observed by him at the Massachusetts General Hospital, in 1836. The individual had only once or twice noticed that his sputa were bloody; he was much alarmed, and, while Dr. P. was conversing with him, endeavouring to divert his fears, he was suddenly seized with violent hæmoptysis, and died almost instantly.

Dr. COTTING said he was called, two years since, to see an omnibus-driver, who was ill with phthisis, but well enough to go out. He was at a stable, looking on at the work, was seized with hæmoptysis, and was immediately taken into his own house, not more than a gunshot's distance, where he was visited in a very short time by Dr. C.; who, however, found him dead.

Dr. JACKSON referred to certain similar instances; but said that, in all he had seen or known of, phthisis was far advanced.

[The Secretary remembers an instance in which the disease was advanced, but the patient was in the habit of being up daily, and had, not long before the fatal bleeding, been out. Summoned in the night to this person, whose residence was only across the street, he was found dead, although not more than six or eight minutes could have elapsed. When the messenger left, the patient was bleeding copiously, but had spoken only a short time previously.]

November 27, 1854. *Membrane, of Tubular Form, passed per Anum.*—Reported by Dr. HOMANS, Sen. The patient was an infant, about six months old in August last, when it was attacked with dysentery, and seen by Dr. H. in consultation. The dejections varied in frequency from ten to thirty-five in the course of twenty-four hours, attended with tenesmus and corresponding constitutional symptoms. They consisted at first of fecal matter with mucus, then of mucus tinged with blood, and finally blood alone was to be seen. On the ninth day the symptoms began to abate, the dejections had become less frequent, and some fecal matter had appeared in them.

On the sixteenth day, after much straining, a mass supposed to be composed of lymph was suddenly protruded from the anus, to the extent of about six inches, according to the report of the attending physician. Efforts were made to remove it by the fingers, but, great resistance being offered, it was cut off by scissors near the anus, a few drops of blood only following the incision. Two or three days after this, a second portion was expelled, by the natural efforts of the child, about one inch in length. Prolapsus ani followed daily for six days, and has since occasionally recurred. The little patient gradually and steadily improved in every respect, and now appears as well as other children of the same age. The mass, after its removal was found to be a hollow tube, which was cut open and then placed in alcohol by the attending physician. It is now about four inches in extent, and of considerable consistence. The surface corresponding to the inside of the original tube is smooth, the other rough, with what appear to be vessels running in different directions. It has been examined by Dr. JEFFRIES WYMAN, who reports it to consist of mucous membrane, vessels, and areolar tissue. He also found what he supposed to be circular muscular fibres under the microscope, and has passed bristles into an artery and a vein.

Singular Movement of the Pupils, corresponding with that of the Eyeballs.—Dr. BETHUNE reported the case. John G., aged 13. Health good, but is "nervous," especially on excitement. The eyes oscillate considerably, as is frequently seen in congenital cataract. This has only been noticed for six years past. The sight is generally good, but not quick, and he is rather near-sighted; and occasionally he has a "blur" before the eyes. On examination, the eyes are generally well-formed and full, but the sclerotica runs into the cornea at the margin. The *pupils play constantly as the eyes move*, dilating and contracting—the right, however, being more active than the left, which is not noticed in the oscillations.

Ellen G., aged 16. Sister of the above. The same malformation at the edge of cornea as in the brother, but the eyes are small and badly developed. She is very near-sighted, and can see neither to read nor sew. She has the same movement of the globes as observed in her brother, but much less in degree, and only of late noticed by the family; also the same corresponding movement of pupils. Her health is good. These are two of eleven children; none of the others have any defect in the eyes, and the parents are not related by blood.

Overdose of Opium in a Child; Olive Oil probably Preventive of Absorption.—Dr. GRAY related the following case, communicated to him by the physician in attendance. E. G., aged 8 years, a resident of Brooklyn, took a dessertspoonful of laudanum, the same being mistaken for tincture of rhubarb. The error was instantly discovered, and the child made to swallow, in addition to a quantity of mustard and salt, four ounces of olive oil. Ipecacuanha was shortly afterwards administered. Dr. S. arrived in about twenty minutes after the opiate was taken; found the patient sitting up, not much alarmed, and apparently free from narcotism. Sulphate of zinc and ipecacuanha, of each a scruple, were directed, causing copious vomiting in the course of ten minutes. The patient then manifested a tendency to sleep, but wakefulness was maintained by conversation. This tendency continued for the space of five minutes only, after which she was perfectly well in all respects. She had repeatedly taken three drops of laudanum, with good results, when suffering from pain similar to that complained of in this instance. The opiate was swallowed when the stomach was empty. Was its absorption prevented by the oil?

December 11, 1854. *Spontaneous Dislocation of the Crystalline Lens in both Eyes, without loss of Transparency; Frequent Prolapsus of the Lens into the Right Anterior Chamber.*—Dr. WILLIAMS presented to the Society a patient, about thirty years of age, whose eyes exhibited the above extraordinary phenomena. She is of feeble constitution. At an early age she had measles, scarlatina, and variola, in immediate succession. Her sight has always been imperfect, and she has been able to see small objects—as in reading or sewing—only when held very near her eyes. On the 8th of June last, she saw a circle—light in the centre, dark around its edges—which interfered with vision in her right eye; but no other change was observed till about a week before she was seen by Dr. W. At this time, in stooping to lift a tub, she was conscious that something occurred in her right eye, and soon experienced circumorbital pain and nausea. She afterward noticed a peculiar appearance in the anterior chamber. During the night these symptoms vanished; but vision was not as good, the following day, as it had previously been. The same phenomena were repeated on subsequent days, after she had stooped forward.

When seen by Dr. W., on the 14th November, in consultation with Dr. Wm. H. Page, the appearances were as follows: The right anterior chamber exhibited the crystalline lens in a perfectly transparent state, resembling, at first sight, a drop of oil between the cornea and the iris. Its weight caused the formation of a sort of pouch at the lower part of the iris, so that the lower edge of the lens was a line or more below the cornea, and its upper edge extended to rather above the middle of the pupil. The edge of the lens was sharply defined, as much so as that of a perfect cataract-glass of one inch focus. No attachment could be seen, nor did the aspect of the pupil indicate that any attachments passed through it. There was no injection of the eye. She had a constant sensation of nausea and discomfort, but less circumorbital pain than after the first prolapsus. Vision was almost abolished; but, on trial of cataract glasses, it was at once improved, so that with those of two inches focus, she was able to read.

As the sight of the other eye was very imperfect, this was also examined. It presented a very marked floating motion of the iris, similar to what is frequently observed after operations for the removal of the lens. Displacement of the crystalline was, therefore, presumed to have occurred in this eye also, and a trial with cataract glasses proved the correctness of this supposition, as she could at once see perfectly. The lens was not visible in this eye; but there was an appearance at the lower part of the iris as if the lens were lying in this situation, and were impelled against the iris as the globe moved. Probably a dilatation of the pupil might have allowed the lens to be seen, but it was not thought advisable to expose the patient to a risk of a similar prolapsus to that existing in the other eye. Visiting her a few days after, when the right lens had fallen back into the posterior chamber, Dr. W. found it equally impossible to perceive any portion of the lens in this, as in the left eye. She required a glass of slightly greater power for the right eye; and, by the aid of these auxiliaries, enjoyed perfect vision.

On the 9th of December, the lens, which for several days had been in the posterior chamber, fell through the pupil, as before, and continued in the anterior chamber till the date of her presentation to the Society. If her head is held forward in sewing, there is no displacement; but it is only in some unlucky moment of stooping, while engaged in active employment, that the lens is projected through the pupil.

The case is of interest, not only from its rarity and the singular beauty of the appearances presented, but as suggesting several questions for solution. It is asserted by M. Magne, in the *Gazette des Hôpitaux*, 10th October, 1854, that all the published observations of dislocation of the crystalline, establish the fact that it can only occur after a direct or indirect blow, or as a consequence of convulsions. But our patient asserts that she has not been exposed to any of these causes. A second point to determine, is whether the lens has or has not escaped from its capsule. If no longer protected by this envelop, we should expect it to become opaque or absorbed. But, though the patient has been four weeks under Dr. W.'s observation, not the slightest loss of transparency or size has been undergone. If the capsule is entire, it must have become detached from its normal adhesions, otherwise the pupil could not contract to a size less than half the diameter of the lens during its presence in the anterior chamber. This could not be the case if suspensary attachments extended from the margin of the lens, through the pupil, to the ciliary circle. The fact of the spontaneous occurrence of the displacement in both eyes, would allow of the supposition that some change has taken place in the vitreous body, depriving the lens of the support usually afforded.

Dr. BETHUNE remarked that, in these cases of dislocation of the lens, he does not remember to have seen opacity of the capsule. This is certainly very remarkable. It is natural to suppose that the capsule would be affected, especially if the dislocation be the effect of a blow.

Chronic Dyspepsia; Cure by Cathartic Treatment.—Dr. BOWDITCH reported two cases in which cathartics had proved of great advantage. The first was that of a seamstress, 64 years of age. She had been liable to what she called a diarrhœa, for some months, having been, before, alternately costive and loose. Under this state of things, she had grown weaker, and a week before Dr. B. was called, she had been confined to her room with colic. On the day of the visit, she had a chill, followed by thirst and fever, dysuria and confinement to the bed. There was an evident soreness, and a hardness in the left hypochondrium. The age of the patient, the long illness connected with the intestines, the tumour in the abdomen, led Dr. B. to suspect some organic disease. It was found, however, on inquiry as to the nature of the dejections, that the *diarrhœa* was, in reality, composed in part, at least, of small, hard, scybala. Dr. B. determined, therefore, to prescribe a gentle but efficient laxative and tonic course. Tinct. rhei and tinct. gentian. comp., each ʒss, twice daily. Eight grs. of blue pill were administered previously to the commencement of this treatment. It is sufficient to say that the cathartic operated gently but effectually. Scybalous masses were daily discharged. Under this, the patient rapidly improved, and after nine visits the tumour had subsided, and with its disappearance the health was soon re-established, so that, at the end of about two or three weeks, she felt better than for months previously.

The result of this case shows the importance of examining *particularly* into the nature of excretions, inasmuch as, although the patient had had a chronic diarrhœa, the scybalous nature of the dejections induced Dr. B. to believe in the retention of small irritating masses in the lacunæ which are often noticed in the large intestine.

The second case was similar to the former:—

An Irish servant girl, aged 27, had been in the Massachusetts General Hospital, in 1851, for catamenial difficulties, of which she was relieved. In 1852, she began to complain of dyspepsia, pain in the left hypochondrium, constipation, &c. At her entrance again into the hospital, in June, 1854, the pain in the side was so severe that in walking she had to press upon the part in order to relieve it. From her entrance till the cathartic course was thoroughly tried, in November, she had similar complaints. The usual remedies for dyspepsia were employed, and occasionally cathartics were given, when costiveness was actually reported. She had dysmenorrhœa, dysuria, &c. Once, there seemed so much uterine trouble, that examinations were made, and the os tincæ was found elongated, but nothing especially abnormal was observed, except an irritation of the vagina. The irritability of the bowels was often complained of, and chalk-mixture and laudanum prescribed. Dr. Bowditch saw her four months after her entrance. Remembering the preceding case, and learning that similar hard masses were discharged, *even when the dejections were said to be loose*, he determined upon a cathartic and tonic course, until no scybala should appear: R.—Sulph. ferri gr. ss; aloes gr. ii, three times daily, ordered on Nov. 5, and continued, with the exception of one day, when the patient had a febrile paroxysm, until 21st. During every day but one or two of that period, the patient had from three to six dejections. Some of them, and generally most of them, were of a scybalous character.

Under this course the patient improved rapidly. She lost her pain in the abdomen, the bowels felt easy, the appetite and strength increased, and she was discharged "much relieved," *i. e.* relieved of many of her abdominal troubles, but still obliged to use laxative medicines, or constipation ensued.

Apparatus for Generating Vapour in Croup.—This was shown by Dr. LYMAN BARTLETT, of New Bedford, Associate Member of the Society. It consists of a common saucepan, and of a tube connected therewith, for conveying the steam, when generated, under curtains surrounding the patient's crib, so that he may be immersed in vapour; a crib can be so curtained by duly covering a clothes-horse, disposed around it. Dr. B. spoke in terms of high praise of this treatment for croup; he had been very successful with it. Other means were, of course, conjointly employed. In from four to five years he had lost only three patients by croup. The vapour is often needed for four or five days; mercurials and small doses of morphia are also employed by Dr. B. There had been no *expectoration* of false membrane; Dr. B. believed it to be often absorbed; occasionally, in his cases, it was thrown off by emesis.

Dr. PUTNAM had treated a case of membranous croup, successfully, by vapour, produced by a common nurse-lamp.

Dr. COTTING had used this latter arrangement; he highly values the vapour-treatment in croup.

Dr. BIGELOW, Sen., referred to the method adopted by the late Dr. FISHER, for vapourizing the air-passages of a patient. Dr. F. was in the habit of tying a bunch of mullen-leaves over the nose of a pair of fire-bellows, then, dipping the mullen in boiling water, he caused the action of the bellows to be continually maintained, in the close vicinity of the patient's mouth and nostrils.

Dr. Bigelow said he had seen some recoveries under this treatment, but also a number of failures.

Dr. ABBOT suggested the employment of the flexible caoutchouc tubes for conveying the vapour.

Dr. TOWNSEND, Sen., mentioned the plentiful generation of steam by immersion of hot bricks in a tub partially filled with water; he had found it of great service, and has lately had a case of membranous croup in which this treatment was followed by recovery. Dr. T. added that steam *may* be so copiously produced by this method that the paper will be removed from the walls of the room.

Calculi from the External Face of both Tibiæ.—Dr. HODGES showed these, which he had found in a cluster upon both the tibiæ of an adult negro woman, a dissecting-room subject; they were situated low down, towards the ankle-joint, and varied in size from that of a small mustard-seed to that of a split pea. Some of them were soft, and crumbled on removal; others were hard as bone. They were imbedded in the cellular and adipose tissue, and had no visible connection with the *veins*. The patient died, probably, from tubercular disease.

Analysis by Dr. JOHN BACON, Jr.—Phosphate and carbonate of lime, with some earthy matter. No concentric laminæ on section, such as are always found in phlebitis. Other concretions had the same composition, and therefore it is difficult to class them with any one special kind of calculi.

Tendinous Fibres between the Phalanges of the Hand.—Dr. HODGES exhibited a hand dissected by himself, in which the above fibres were distinctly shown; they are strong and tendinous, found just beneath the skin, and imbedded

in the adipose tissue; they always exist in this situation and (what has probably prevented them from being often dissected), form, as it were, the *skeleton* of the rudimentary *web* existing between the fingers. They extend about half an inch in width; the fibres thickly interlacing with each other and losing themselves on the palmar aspect of each finger half way down the length of the first phalanx; the fibres coming from one side of the finger crossing those from the other at its base, to be distributed on the opposite half of the phalanx. They have no connection whatever with the palmar fascia, and exist about equally well-marked in all hands.

Dr. H. remarked that neither Cruveilhier, Sappey, Sharpey, Quain, nor Wilson, or other authors to whom he has had access, figure or describe these fibres.

Encephaloid Disease about the Scapula; Malposition of the Uterus.—Dr. J. B. S. JACKSON showed the specimens; fragments of the bone, which, to a considerable extent, was quite destroyed; an encephaloid mass of moderate size, from about the bone, and one from the axilla, which last is about half as large as the fist. The cartilage over the glenoid cavity is almost intact; as it is so often observed to be in the case of other bones when affected with cancer. The head of the humerus appears healthy externally; but, on section, shows a cancerous deposit within; as does also the sternum. The lymphatic glands in the upper part of the abdomen were also diseased; with traces of the same in the breasts, and upon the surface of the lungs; there being in these last, the central depression in one or two of the deposits that is so often seen in cancerous disease of the liver.

The patient was a very healthy-looking girl, æt. 25. Swelling was first noticed nearly five months before death, and steadily increased. Pain at first dull, but, for the last three months, quite severe; with much discharge for several weeks before death, and a general decline of the health.

The uterus, in this case, lay horizontally and almost transversely across the pelvis; the os uteri being directed towards the left hip, and the fundus lying directly against the right ovary; the ligament of this last being quite short, whilst that of the left ovary is as much too long. The cervix is not bent upon itself, and the organs are seen to be otherwise perfectly normal.

Upon her entry into the hospital this patient reported the catamenia as regular; and, upon inquiry of the nurse who had charge of her for three months before her death, it is ascertained that the function was in every way perfectly performed. Dr. J. has recently met with a case of very complete retroversion, and in which the fundus lay about opposite to the right edge of the third sacral vertebra; state of the catamenia unknown. He seems to think the importance of these malpositions very much over-estimated.

January 8, 1855. *Union of both Leg-bones by Callus after Fracture; Fracture across Base of Skull, with extensive Laceration of the Cerebral and Cerebellar Substance resulting from a Fall.*—Dr. CABOT showed the leg-bones entirely united together by callus; the specimen was sawn longitudinally, and the bone shown, continuous throughout. The cause of death being interesting, Dr. C. described the case. T. C., 35 years old, a healthy-looking Irish labourer, was brought into the Massachusetts General Hospital, December 29, 1854, P. M., reported to have fallen into the hold of a vessel. At first, he was quite delirious, throwing himself wildly about, and it was necessary to restrain his motions by strapping him to his bed. He finally became quiet, fell asleep, and died about 5 o'clock A. M. of December 30th, having

lived sixteen hours. There had been free hemorrhage from the right ear, and from the nose; both upper eyelids largely ecchymosed.

Post-mortem Examination.—Upon removing the brain, a fracture was found extending along the right side of the base of the skull, running from before backwards, from about the lambdoidal suture, the whole length of the base, and across, apparently through the ethmoid bone, and appearing on the left pterygoid process of the sphenoid bone. There was also a fracture from before backwards through the centre of each orbital process of the frontal bones. The right lobe of the cerebellum was completely "mashed," as it were; the right anterior lobe of the cerebrum was extensively lacerated, and the left slightly so; there were several other slight lacerations about the base of the right hemisphere. Right ventricle largely torn, with a considerable clot of blood. Copious effusion of blood, coagulated, was found over the base of the brain.

The prolongation of life, after such very great injury, was probably owing to the free escape of blood from the nose and ear.

Cancerous Disease of the Œsophagus.—Dr. CHARLES E. WARE presented the specimen, and gave the following account of the case: The patient was a woman, 42 years of age. For five or six years she had been subject to an embarrassment about the larynx, which disturbed her, particularly in her sleep, producing a peculiar sonorous sound, different from that which ordinarily attends sleep. During nearly the same period, she had had occasional difficulty in swallowing, which would sometimes force her to rise from table and clear her throat. When Dr. Ware was first called to her, the 3d of March, 1854, she said that, at that time, she had no difficulty in swallowing. She only complained of uneasiness about the larynx, some cough, and her sleep disturbed by the noise she made in respiration. She was always a pale, colourless woman, but she then looked more blanched than was natural to her. In the course of two or three weeks, it was evident that there was some trouble in swallowing, and that it was increasing, although it was not until the middle of April that it was sufficient to prevent her from taking her usual amount and kind of food. Her cough continued to increase in severity, and was particularly urgent on lying down at night. It was accompanied by a copious frothy expectoration and great irritation about the neighbourhood of the larynx. Her catamenia, which had been irregular for about two years, appeared scantily for the last time in April. By the 6th of May, she could swallow nothing dry or solid. Then a staff, with an ivory bulb three-eighths of an inch diameter, was passed with some difficulty and pain. The obstruction was found to be at the upper part of the œsophagus. In the course of three weeks, passing an instrument every three or four days, the passage was so much dilated that an instrument a third larger could be passed, and deglutition so much improved that she left town on a journey. She was absent four or five weeks, and, on her return, her œsophagus was more contracted than at any time. She was only able to swallow liquid food, and this annoyed her exceedingly by pouching at the side of the stricture. She had a great deal of pain about the back of the neck and through the ears, but not much pain or tenderness on external pressure about the immediate neighbourhood of the disease. The instrument was passed again, but with more pain than at first, and without benefit. It was continued until the middle of August, when it was abandoned on account of the suffering it occasioned. She continued to lose strength and flesh very slowly, and to be forced to more and more diluted nourishment. During the last few months she suf-

fered very little from pain or cough. Both seemed to subside as the disease advanced. Towards the close, she suffered most from the accumulation of viscid mucus above the stricture, and which she raised with difficulty. Sometimes the mucus contained blood and pus, and was fetid. But this was not its most common character. She died January 5, 1855. As there is always during life an uncertainty in the diagnosis of these cases, the treatment must be based upon the possibility of their being simple stricture. And this was the course pursued with the present case. Instruments were used as long as they could be borne, and any benefit could be obtained from them. On the return to the instrument in July, it was found necessary to use one with the staff projecting beyond the bulb to act as a director. Otherwise, it was almost impossible to pass the instrument into the stricture. It would lodge by the side of it. It was also attempted to keep the stricture on the stretch for awhile by passing an instrument with a bulb of a dumb-bell shape, allowing the instrument to rest with a swell of the bulb at each end of the stricture. This, on account of the pain it occasioned, was not long persisted in. During the last few months, the treatment was simply alleviative. After all solid food was abandoned, the meat biscuit, made into soup, was found to be the most nutritious and satisfactory diet, and it continued to be acceptable the longest. During the last few days of her life, she was sustained entirely by nutritious injections.

Dr. J. B. S. JACKSON communicated the following account of the appearances observed at the *post-mortem* examination:—

On dissection, there was found a well-marked encephaloid disease of the pharynx and œsophagus, extending from the top of the arytenoid cartilage to the lower extremity of the thyroid gland. The disease was remarkably defined; the posterior and lateral portions being affected, whilst the anterior, with the glottis and the larynx, was perfectly healthy. The diseased structure was from one-fourth to half an inch in thickness, white, soft, and upon the inner surface, uneven; the passage through it being sufficiently free. Just above the upper limits of the disease, and upon the left side, was the opening of a cavity that burrowed down between it and the larynx, and that would have held, perhaps, two drachms; it was offensive and somewhat dark, though not fairly gangrenous, but it contained no proper pus, nor any appearance of food. The other organs were healthy.

"Gaseous Cysts" upon the Mesentery and Intestine.—Dr. H. J. BIGELOW exhibited the intestines and mesentery of a hog, which he had received from a butcher, and which resembled one which Dr. B. had shown to the society a year or two before. An almost precisely similar specimen existed in the Hunterian Museum, and is engraved in the works of Mr. Hunter. Much prominence has been given to this specimen by Mr. Paget in his work on Tumours, being the only one which he refers to under the head of "Gaseous Cysts;" the specimen, moreover, had been examined by Mr. Cavendish and by Dr. Jenner, who sent it to Mr. Hunter. The present specimen shows innumerable air-bubbles beneath the peritoneum, especially at the junction of the mesentery and intestine, and throughout a large portion of its length. They vary in size from a millet-seed to a large green pea, are thickly studded, some of them agglomerated, others solitary, some imbedded in the fat, and others exceedingly delicate, transparent, and hanging off from the peritoneal surface by a slender pedicle of some length.

Dr. B. said that, in the absence of any satisfactory explanation of the mode of production of these air-cells, he would allude to a precisely similar

appearance in the human subject that he had observed and brought before the Society some years ago. The patient was a boy, about ten years of age, and died under the care of Dr. Phelps, of Attleboro', from whom Dr. B. had received the specimen. In that case, the air-vesicles were numerous, and identical with those above described; but the stomach presented an enormous dilatation consequent upon an old contraction at the pylorus. Precisely at the pylorus there also existed an ulcer of considerable extent, which might readily have engaged beneath its mucous surface a part of the air which was forced from the stomach into the duodenum, thus producing a sub-peritoneal emphysema. In the present case, however, there was no ulcer in the stomach or duodenum, which were opened and thoroughly examined; and about which last there were as many vesicles as about the intestine below. In the case of the hog first examined, the stomach was not seen by Dr. B. It is stated by Mr. Hunter that the hog is subject to ulceration of the alimentary canal during the summer months; and it is said by the butchers that the appearance of air-cells, as above described, is not of rare occurrence. In the course of the discussion upon this case, Dr. Jackson alluded to one that he met with some years ago, of chronic ulcer of the duodenum, and which caused death by perforation; besides the peritonitis, there was found an extensive emphysema of the sub-peritoneal cellular tissue, very much as in the foregoing cases.

Evidences of Arrest of Tuberculous Disease in the Lungs; Cicatrization, Cretaceous Masses, &c. &c.—Dr. ELLIS showed the specimen, and gave the history of the case. The patient was a printer, 52 years old, who, since the age of 18, had been subject to attacks of hæmoptysis, a year sometimes elapsing between them, but the interval was usually much shorter. The quantity of blood lost was perhaps only sufficient to tinge the sputa, but frequently amounted to Oss. There was also a "wheezing asthmatic cough," with dyspnœa and palpitation of the heart on exertion. He, notwithstanding, attended to his business until three months before his death, weighing, by estimate, about 125 pounds. After being exempt from the hæmoptysis one or two years, it again returned a month previous to his death, and increased until Oss of blood would be lost at a single hemorrhage; and this would perhaps be repeated during the day. After persisting in this manner several days, it ceased a week before his death; but, apparently exhausted by the repeated losses of blood, he sank, and died on the 19th of December. The above particulars were all that could be obtained, nothing being known about the physical signs.

Autopsy, about twelve hours after death.—Externally. Cadaveric rigidity well marked. The chest appeared longer than usual. *Pleuræ.* Surfaces of both pleuræ universally adherent, the uniting tissue over the greater portion being very thin and delicate, but at the apices very thick and strong. The false membrane at the latter points was from one to two lines in thickness, of a dull-white colour, very firm, dense, and contained much fat, which gave it the appearance of adipose tissue. The trachea and bronchi were considerably dilated and thickened, and contained some muco-purulent fluid. *Lungs.* The upper part of each lung, to the depth of an inch and a half from the summit, corresponding to the thick false membrane mentioned, was converted into a mass of dense fibrous tissue, in the midst of which were seen the dilated bronchi, in which the mucous membrane was raised in the form of transverse valves or ridges; a few small cretaceous masses; and a number of cavities, several of which contained caseous or purulent matter, but the greater part of them were quite smooth on the inner surface, particularly the largest, which measured six-eighths of an inch in diameter, and was lined with a red

membrane, very closely resembling the mucous. A bronchus was here seen, which terminated abruptly, its extremity being rounded off, forming a cul de sac. The rest of the pulmonary tissue below the diseased apices was healthy, with the exception that it contained quite a number of isolated tubercles scattered about in its substance, and also a cretaceous mass, two or three lines in diameter, around which the lung, for some distance, had a puckered appearance, as if drawn in by the contraction of a deposit much larger than that which existed at the time of death. Heart rather large, but otherwise normal. Other organs not remarkable. Brain not examined.

January 22, 1855. *Screw ejected, after Six Weeks' Retention in the Neighbourhood of a Child's Larynx.*—Dr. C. E. WARE reported this case, the history of which he received from the patient himself, now a vigorous man of about 30 years. The patient, when three years old, picked up the screw from the floor, and, putting it in his mouth, by accident swallowed it. The screw was such a one as is used in fastening brass plates on bedsteads.

For six weeks, at intervals of a few days, there would be an access of very severe dyspnœa, with cough of the character of croupy cough, and discharge of frothy mucus, mingled with blood, and, as is stated, purulent matter; after which, for about a week, ease would be obtained. Emaciation and weakness became extreme; it was supposed by the attending physician that the patient was in consumption. The child not talking plainly, and saying it had "swallowed a *skewer*," much obscurity attended the diagnosis of the case. During one of the fits of dyspnœa and strangling, the patient's mother, much alarmed, held him bending forward, while in the act of coughing, and, as the patient writes, "out flew the screw, covered with rust;" and, immediately after it, "half a pint of blood and matter."

The patient did not fully recover for nearly a year.

Dr. CALVIN ELLIS exhibited the following specimens, and gave accounts of the cases:—

CASE I. *Induration of the Brain in a Child.*—The child from which the specimen was taken was healthy at the time of its birth, and continued so for two weeks, when contractions of the hands and feet were noticed. At the same time, it became very irritable, and restless at night, and soon began to lose flesh, although continuing to nurse. It finally became unconscious, and remained so, constantly moaning and crying, until death, which took place at the age of nine months and three days, a sister having previously died, at the same age, with the same symptoms. During the last three weeks there was some cough. The above account is necessarily imperfect, as the attending physician was not seen.

Autopsy, twenty-six hours after death.—Externally. Great emaciation; no cadaveric rigidity. *Head.* Sutures closed; a small membranous point alone remained of the anterior fontanelle. Dura mater universally adherent to calvaria, but separated without any great difficulty, except at the sutures. Its necessary removal with the bone allowed the escape of ℥vj or ℥viij of yellow serum, which had collected in the cavity of the arachnoid, filling a space about an inch in depth between the cranium and the surface of the brain. The membranes covering the convexity of the hemispheres normal. No unusual amount of serum under the arachnoid, nor in the lateral ventricles. To the feel, the brain was very much firmer than usual, but the surface was not involved, since the cortical substance could be scraped away, exposing the condensed portion below, which retained the form of the convolutions. The

brain was removed for a more accurate examination than could be made at the house, but it unfortunately softened so much in a short time that the exact limits of the disease could not be ascertained. It may, however, be stated, in general terms, that the white substance was very extensively, though not uniformly, affected, the induration being as well marked in the cerebellum and optic nerves as in the cerebrum. The consistence of all of these parts was somewhat greater than that of the pons varolii in the normal state. Their colour was everywhere the same, viz: bluish white, with perhaps some bloody points, and the cut surface smooth and shining. Spinal cord normal. *Lungs.* Some limited pneumonia in each lung, but particularly in right lower lobe. Liver light-coloured, of large size, and very fatty. Other organs not remarkable.

Case II. *Glandular Proliferous Cyst, with Hypertrophy of the Breast.*—The breast was removed by Dr. Hayward, Sr., from a woman 37 years of age. The disease had existed a year, and, towards the close, became quite painful. No retraction of the nipple.

On the external surface was a small opening, looking like that of an ordinary abscess. Breast of small size, but imbedded in, and firmly attached to, various parts of the gland, were a number of hard nodules, the largest of them half an inch in diameter, which, on incision, yielded a milky juice, and presented an appearance not unlike that of common scirrhus, with the exception that they were somewhat lobulated. At one point, where fluctuation was detected, a cyst was opened, more than an inch in diameter, with a perfectly smooth lining membrane, attached to which, by broad pedicles, and nearly filling the cavity, were cauliflower-like growths, somewhat lobulated or foliated in their structure, and much less firm than the nodules previously mentioned. Another cyst, filled with a sanious fluid, was opened during the operation.

On microscopical examination, by Dr. H. J. Bigelow, there were found numerous large free nuclei, with large nucleoli, many large irregular cells, and also a few compound cells. With a low power, a glandular structure was seen, composed of a number of uniform, rounded masses, the interior of which was occupied by the nuclei and cells described.

This hypertrophy of the breast is an exuberance of the glandular structure, with a little deviation from its normal character, and not the hypertrophy of the fibrous tissue of the ultimate lobules which is a distinct variety.

Case III. *Disease of the Liver.*—The patient was a gentleman, 53 years of age, who formerly resided for many years in Pernambuco, but during the latter part of his life had lived in the vicinity of Boston. Always well until two years ago, since which time he had been troubled with ascites and anasarca. No pulmonary nor cardiac symptoms. Never had any pain in right hypochondrium, nor yellowness of skin, according to the report of one who had seen him during life, but there was yellowness of the face after death. Urine examined a number of times, but no albumen ever detected.

Autopsy, twenty hours after death.—Externally. No cadaveric rigidity. Emaciation well marked. Abdomen flat, but fluctuation very distinct. No œdema of lower extremities noticed, this having disappeared, as was stated, before death. Head not examined. *Pleuræ.* The left contained ℥ii of serum; the right, Oj. No adhesions. Lungs congested posteriorly; lower lobe of right considerably compressed; otherwise nothing remarkable. Heart of small size; left ventricle firmly contracted. *Stomach and Intestines.* Small intestine contracted; otherwise nothing remarkable. Spleen of usual size. Surface wrinkled, with a whitish macerated look. Substance firm; of a chocolate-brown colour. Kidneys quite firm; normal. Bladder normal. Peritoneal

cavity contained about a gallon of greenish-yellow serum. Diaphragm very flaccid, as if from over-distension. In this connection, it may be well to state that the size of the abdomen had diminished very much before death. Liver of a dark-brown colour, six inches long, five broad. Surface smooth, with the exception of a portion, about two inches in diameter, immediately above the gall-bladder, where it presented the appearance of an old cicatrix, in the midst of which were seen a number of yellow nodules, the colour of which was owing to the presence of dense yellow fibrous masses, of irregular shape, imbedded in, and firmly attached to, the substance below, the latter being, for some distance, in the same cicatrized state as the surface. Several similar yellow masses, a few lines in diameter, were seen in other parts, through the capsule, but no cicatrization had taken place around them. The substance of the organ was very firm, and on the cut surface had a granular appearance, which was still more strongly marked when the organ was torn. The fibrous tissue did not appear unusually abundant, except around some of the branches of the vena portæ; neither were the edges of the liver sharp, nor the external surface nodulated, as in ordinary cases of cirrhosis. Under the microscope, nothing was noticed in the yellow masses but amorphous matter, small oil-globules, and a few small scales of cholesterine. Bloodvessels, where they entered the liver, normal, as were also the bile-ducts. *Gall-stones.* In the gall-bladder, which contained a small quantity of dark green bile, were quite a number of blackish gall-stones, the smallest two or three lines, the largest an inch and a half in diameter, all rounded, but marked with facets.

Fibrous Polypus of the Uterus, ten inches in circumference, five inches in length, with a Pedicle half an inch in diameter.—Dr. Putnam presented the specimen, and reported the case of a patient he had visited, in a neighbouring city, with Dr. Channing. She was 41 years of age; had one child æt. 21. Began to flow nine years since, chiefly at the menstrual period; but in the interval also, whenever she made any exertion. Had latterly been much reduced by the loss of blood, and was confined to her chamber. One quite unusual symptom was a painful distension, caused by the accumulation and gradual wedging of the coagula between the tumour and the walls of the vagina. This, at times, became so intolerable that she was compelled to have them removed.

Dr. C. passed a ligature by means of Gooch's canula, and the tumour came off on the fifth day.

Intermittent Fever; Homœopathic Treatment; persistence and aggravation of the Symptoms; Treatment by Quinine and other usual means; immediate arrestation of the Chills; rapid recovery.—Dr. Morland read the following account. It is well known that homœopathists adduce the curative action of cinchona in intermittent fever, as "one of the strongest possible proofs" of the truth of their doctrines; in fact, "it was in attempting to ascertain how cinchona cured ague or intermittent fever, that Hahnemann made his alleged discovery." While experimenting upon himself with the bark, "an intermittent fever ensued," and he came to the conclusion that the drug must cure intermittent solely by its power, as he asserted, of producing like symptoms in a well person; and hence his deduction "*Similia similibus curantur.*"

Professor Simpson (*Homœopathy: Its Tenets and Tendencies;* Edinburgh, 1853) has well shown the fallacy of the premises upon which Hahnemann founded his system of practice; and he has abundantly proved it not to be true that intermittent symptoms are, by any means, invariably produced, as above asserted; and not only so, but he shows that such an occurrence is a

very rare and quite an exceptional thing. We can readily suppose that, if the very imagination which suggested in Hahnemann's mind the idea which serves as the motto for his system be so utterly fallacious, the attempts to *cure* intermittent fever by infinitesimal doses of quinine, would signally fail; the endeavour itself thus becomes a severe test of the practice; and even homœopathists confess their want of success. (*Vide* SIMPSON, *op. cit.* p. 248, Edin. Ed.) How absurd, in view of such facts, do the confident assertions of the majority of homœopathic practitioners become, such as the dogma that "if any remedy be homœopathically selected, it *will* cure," in whatever dilution it be given?

By their own showing, cinchona and its extracts are "homœopathically selected," if administered in intermittent fever; why should they ever fail in their hands, particularly when the medicine is properly and sufficiently *diluted*, to adopt their, to us, ridiculous phraseology.

In the serious matter of compromising a patient's existence, by a do-nothing course, in a case demanding the most decided remedial interference, it is unfortunate that the public cannot be enlightened as to the responsibility incurred, and the neglect practised, not only by the practitioner who persists in leaving unassisted Nature to struggle with an opponent sure of gaining the mastery, but also by themselves, in allowing their friends thus to be left to their fate.

With a view of illustrating, in some degree, these positions, the following account of a case has been prepared.

Mrs. ———, a young lady of delicate constitution, and for several years a resident in a tropical climate, had, during the latter part of 1853, while at the South, an attack of intermittent fever of the tertian type, and of but slight intensity; it readily yielded to quinine. She came to the North, in January, 1854, and by a fatiguing journey; being far advanced in pregnancy with her third child; fatigue and apprehension caused by accidents during the journey, nearly produced miscarriage soon after her arrival, and she suffered from weakness and cough, until the period of her confinement, February 26, 1854, having completed her full term. The access of labour being somewhat sudden, a midwife was first in attendance; there was retained placenta, and profuse and exhausting flowing subsequently, which, supervening upon her previously weak condition, reduced her to an alarming state; she was perfectly bloodless in appearance; greatly emaciated; her pulse rapid and feeble. She, however, rallied; and, in from three to four weeks, went down stairs to dinner. At this time, a most unfortunate epoch for the advent of new trouble, chills, followed by fever, came on, and a regular *tertian* was declared. She was attended by a homœopathic practitioner, who administered various infinitesimal doses; and, finally, but not until the *tertian* had become *quotidian*, gave quinine in so diluted a form (stating it to be the "first dilution") that, when subsequently asked by the physicians who finally managed the case, how much he gave of the salt, he was unable to say. Under this course the patient continued to grow weaker; the chills recurred, with violence, every twenty-four hours, and generally at an early hour of the morning; after each attack the patient evidently had less and less power of resistance. On the arrival of her husband, who had been absent during this last illness, he immediately dismissed the attending practitioner, remarking that, even to the eyes of persons unskilled in medicine, it was sufficiently evident that Nature unassisted, could not, in *this* case at least, do the work of cure—however possible such a result might be in a robust person.

On Tuesday, April 4, 1854, I was desired to take charge of the patient; but, in view of her then almost hopeless situation, declined to do so unless

with a previous consultation; which being consented to, Dr. BIGELOW, Sen., saw her with me, at about noon of the above day. There had been a severe chill about ten or twelve hours previously. Dr. Bigelow expressed strong doubts as to her recovery, taking into consideration the previous history and her present very weak and alarming condition. It was, however, resolved to give *quinine* in as large quantity as the system seemed likely to bear, combined with nourishment sedulously and judiciously given (the latter point had been, in good measure, attended to previously); the administration of the remedy was immediately commenced—two grains every two hours; at a second visit, same day, P. M. I directed brisk friction with warm laudanum, just before the *expected* chill, with fifteen drops of the same internally; *dry* warmth to be afterwards employed (in place of wrapping the patient in a blanket soaked in warm water, as was done by the former attendants), and the said friction to be kept up as long as there seemed even a tendency to chill.

April 5, 10 o'clock A. M. *There had been no chill;* patient expressed a sense of comfort at her escape from it. Pulse, about 110, feeble; skin natural. *Perge.* The same course of treatment was pursued, with the same, and with constantly better results, as regarded the patient's progress towards recovery. Much of the success attained is doubtless to be ascribed to the untiring exertion of the patient's husband in giving the quinine regularly, securing the prompt administration of food, and making the frictions with his own hands; these latter, either with warm spirit, laudanum, or with the dry hand, were resorted to on the least feeling as if of threatened chill. There was, however, no recurrence of the chills which, before the change of treatment, had been so regular of access. The quinine was continued for two weeks; for the first week in the dose above stated; during the second it was gradually diminished, and finally suspended. Citrate of quinine and iron was given pretty freely during convalescence, which was rapid, when the extreme prostration is considered. The pulse, three or four days after commencing the quinine, sank from over 100 per minute, to 96, 90, 80; it gained strength and regularity daily; the appetite became strong; the digestion was good; colour returned to the previously white lips. In the second week of treatment the patient could walk across the floor, with assistance (ten days before this she could scarcely lift her hand), in a few days she was out, and in a very short time, she whose chance of life was pronounced "*not worth the toss of a dollar,*" went from Boston to Washington, D. C., bore the journey well, grew stronger and gained flesh rapidly, has since gone to Europe, and by late accounts is quite well.

I firmly believe that what is said by Dr. BARTLETT (*Treatise on Fevers*, 1847) of the "*congestive form*" of periodical fever, would have proved true of this case of simple intermittent, had the same course been continued under which it became so grave. Dr. B. says: "*The paroxysms must be arrested or the patient will die; the only agent in our possession, by which this can be done, is the bark* (cinchona) *and its preparations; and no time is to be lost in their use.*" (*Op. cit.* p. 391.) The question *will* arise, how any well-educated practitioner (in this instance the homœopathist in attendance was such) could, in conscience, allow the disease to progress, when, to his knowledge, he had the means of arresting the paroxysms at hand. One more quotation seems so apposite that it may be admitted: "All that respects the disease, and all that respects the remedy, is so marked, so sudden, and so forcible, that physicians neither doubt nor reason about the matter. They *see* what happens, and, resting upon the evidence of what they see, they know that the disease is cured by (mercury)" quinine. (Latham *On the Heart*, vol. i. pp. 266–7.) Dr. Latham is speaking of mercury—by substituting *quinine* the sentence is quite in place.

It is often remarked that one isolated case proves nothing; granted—yet an aggregate of such cases will surely prove something; and amidst the boasted "cures" of the homœopathists, it seems but simple justice that a counter-report should occasionally be made. To most legitimate practitioners, however, such cases, singly, must carry their own evidence.

At the next subsequent meeting (February 12th, 1855) Dr. GOULD related the following case, of which he had been reminded by the above:

A gentleman from the state of New York, on a visit to Boston, had an attack of tertian, and placed himself under homœopathic treatment. The paroxysms grew more severe, and became quotidian. Consultations were held, but no abatement was experienced; and, after two weeks, being very much exhausted, it was concluded, by both patient and doctor, to abandon that method of treatment. On surrendering his patient, the physician remarked, that he presumed that the disease would be soon arrested, as it was well known that quinine would control fever and ague. Being asked, why then did he not employ it, he replied that it was not in accordance with their doctrine, and therefore he preferred not to try it.

On visiting the patient, he was found to be deeply jaundiced, and his liver protruding from under the ribs; bowels constipated. Blue pill was given, and hot fomentations were applied to the hepatic region. The bowels were freely evacuated, discharging large quantities of bile. One paroxysm, only, occurred subsequently, and the recovery was very rapid and complete. No quinine was given.

[The statement of the homœopathist, in this instance, that the use of quinine in intermittent fever is "not in accordance with their doctrine," only serves to expose his ignorance of his master's teachings, and indeed of the *basis* of the "doctrine;" as noticed in the remarks prefatory to the first case.—*Secretary.*]

Dropsy, supposed Ovarian; its disappearance after copious Diuresis.—Dr. WM. E. TOWNSEND related the case. The patient had had spinal irritation, and had kept her bed for fifteen years, from this cause; there being actual inability to maintain the erect position. The cysts, said Dr. T., could be distinctly felt; the patient having become very thin. Last summer she had disease of the heart, and also dysentery, which latter became chronic. Death occurred on Saturday night last (20th inst.). The body weighed only forty pounds. The dropsy was in all probability ovarian, having lasted for a period of five years from its commencement to its termination, and completely disappearing after very abundant diuresis. No *post-mortem* examination was allowed.

ART. IV.—*Cases of Fistulæ, communicating with the Gall-bladder.*

By SAMUEL KNEELAND, Jr., M. D., Boston, Mass.

CASES of fistula communicating with the gall-bladder are alluded to in the books and journals, but not with sufficient details to render the symptoms and pathology intelligible. Cases are occasionally found in the books like the following from *Heberden's Commentaries:*—

"Case I.—A woman, 50 years of age, was for ten days severely afflicted with pain in the stomach, hiccough, purging, and fainting. A month after, a swelling arose near the navel, which was opened, and discharged a great quantity of yellow fluid for the space of four years; at length the pain increased, with sickness and shivering, and after a few days there was discharged a gall-stone, three inches long and as much in circumference, weighing 245 grains. During the following two weeks, a thin liquor was freely discharged; the sore soon after healed up, and the woman recovered."

Case II.—This is taken from vol. 16 of the *Surgical Records of the Massachusetts General Hospital*, June, 1837:—

H. E., female, æt. 55; married. Lived three years in Lowell; health in youth pretty good; is the mother of eight children, all living; ceased menstruation in 1830, at the age of 48. A short time previous to that, began to feel an uneasiness in right side; a kind of aching and smarting, extending sometimes to the epigastrium; with a throbbing under the cartilages of the right ribs, where a small hard tumour could be felt, as if deep within and a little upward under the ribs. Much oppressed by food, but no vomiting; frequently troubled by cold chills; skin never yellow. Pain and swelling in side worse in winter; bowels often much distended, distension coming on suddenly, obliging her to loosen all her clothes; could not wear clothes tight on account of soreness of side. About three years ago, exerted herself much, when a soreness was felt in side, extending down to right iliac region, where a hard body could be felt. At night, unable to lie on right side until the swelling and pain had subsided. Bowels usually costive, requiring medicine; when no dejection for two or three days, had "cramp" about right short ribs, at times very severe. In October, 1834, had "typhus fever," which kept her sick two months; towards the end of disease, had bloody discharges from bowels. In January, 1835, the swelling much increased, frequently extending over half of abdomen; at one time very great, with severe pain passing to left side; in the course of the following night the pain passed off, leaving a soreness in right iliac region, preventing a free motion of leg. The next month, after another attack of pain, became suddenly yellow; the next day, after nausea all night, a discharge of blood took place from the bowels. During the following summer and autumn, was subject to frequent pains in side, but during the intervals felt herself getting better. About sixteen months before entrance, an opening appeared about a quarter of an inch to the right of the umbilicus, discharging a watery fluid; the tumour gradually got more towards the umbilicus, and was more distinctly felt under the parietes. During the last summer, pus was discharged at times; sometimes a fluid like "clear gall," staining whatever it touched. About three months before entrance, a substance like a gall-stone was discharged. This stone is in the cabinet of the Boston Society for Medical Improvement, and is numbered 566; it was about equal to an inch in diameter, and much worn at one extremity. Since then, the opening has never closed; the discharge is small; twice since the discharge of the stone, a yellow, gall-like fluid has flowed from opening. During the last winter, the bowels have been very costive, requiring medicine often; strength much diminished; appetite good, with no trouble whatever from food since the discharge of blood in February, 1835; sleeps well; complains of pain between shoulders and at epigastrium.

Condition on entrance.—Aspect good; skin not yellow, cool, and soft; respiration natural; no cough; pulse 72; tongue clean. Complains of pain between scapulæ, and of a prickly sensation just to the right of umbilicus; had a similar sensation before the last gall-stone passed. Around umbilicus,

and at right side, are the remains of the cicatrix, with a fistulous opening; a probe passes into the fistula, to the right and a little upwards, about one and a half inches, in a direction nearly parallel with the integuments, striking a body evidently hard; a slight tumor perceived externally in this spot, a little movable; flatness of percussion in the right hypochondriac region, extending down nearly to the ileum; the edge of the liver is felt just above the crest of the ileum, quite perceptibly. Appetite good; not troubled by food; one dejection yesterday; urine well; slight discharge from fistula.

She remained in the hospital sixteen days, during which poultices were applied twice, and the opening dilated with a sponge tent. The discharge became more profuse, rather thin, and there was considerable pain and smarting about the fistula, with some tumefaction; otherwise well. The stone was gradually advancing to opening, when she asked for her discharge.

In this case, the liver was considerably prolonged downwards, perhaps increased by the use of corsets; but, whatever the cause, the same will account for the low situation of the fistula.

Case III.—This case I first saw in 1847. The patient, a man aged 77, had enjoyed almost uninterrupted good health till within a year, and had led the laborious life of a New England farmer; he had been at times intemperate and irregular in his habits, from which his health, however, had not apparently suffered; at that time he was remarkably vigorous for one of his age. In 1846, he was attacked, without any known cause, with bilious vomiting, accompanied by colic pains of considerable severity; in a few days appeared in the right hypochondriac region what he thought was a boil, which swelled, and opened itself, when a gush of bile and afterwards a serous fluid flowed out, with a gall-stone of the size of a beech-nut; in a week came out another stone of the size of a pigeon's egg; and, in the course of a few weeks, two others of smaller size. The passage of the stones was accompanied with pain and a pricking sensation. The only treatment employed was a sponge-tent to dilate the opening, and poultices occasionally. The discharge varied in its character from a thin, serous fluid to apparently pure bile. He has never had any trouble in the digestive, urinary, or respiratory organs; has never had jaundice, to his recollection, nor clay-coloured stools; the bowels generally have been regular; few men have been more free from indisposition than he has been through life.

In 1847, when I first saw him, there was a fistulous opening, with irregular and somewhat irritated edges, situated about two and a half inches above, and four inches to the right of the umbilicus. The opening would admit the end of the little finger, and with a probe could be felt a hard substance, about an inch and a half from the skin; by a slight dilatation, could be seen a whitish calculus, about half an inch in diameter, and quite smooth; the discharge was slight, and of a serous character. The stone came out in the course of three months. The fistulous opening then healed, with an irregularly depressed cicatrix.

The patient, having sprained his ankle severely, came to Boston to consult an eminent surgeon, about four years after this. The surgeon disbelieved the old man's statement of the case as above given, at which he was much vexed. In 1852, I saw him again, when he told of the surgeon's incredulity, and of his affirming that such a thing was never heard of; at that time the fistula remained closed, but a sense of pain and pricking in the part told him another calculus was on its passage. I lost sight of him after this, but have been told

that the calculus passed in the same way as the others. All the stones that were passed have been presented to the Boston Society for Medical Improvement.

A chemical and microscopical examination of these calculi was made by Dr. B. S. Shaw, with the following result: Calculi seven in number—three measuring a quarter of an inch in diameter, three measuring three-quarters of an inch in diameter, and one measuring five-eighths of an inch in diameter.

The largest was externally of a dirty-white colour, with spots of green and yellow where the outer coat was worn or broken off. The others were green or greenish brown externally, and of the colour of ordinary biliary calculi. They were all covered with smooth convex, concave, or plain faces, having evidently once been articulated with each other.

On section, they presented a central nucleus, or a cavity in which a nucleus of soft and moist nature had dried and shrunk. This nucleus, in the larger calculi, measured one-fourth of an inch in diameter, and the cavities measured the same. It was composed of crystals of cholesterine, and inspissated biliary matter. The substance of the calculi was yellow, with spots of green, and coated with a layer of dark green, to which their colour externally was due. The dirty-white coat on the largest calculus was found to be composed almost wholly of cholesterine.

The substance of the calculi was composed principally of cholesterine, and the different shades of colour were owing to greater or less quantities of biliary colouring matter. The powder resulting from the section of the calculi, and containing a portion of each layer and colour, gave, on analysis:—

Cholesterine	84 parts.
Biliary and colouring matters	10 "
Inorganic matter	6 "
	100 parts.

The specific gravity, also, of these calculi was carefully taken, and found to be 0.812. This is of considerable interest, as the specific gravity is rarely, if ever, noted.

Case IV.—In the London *Medical Times and Gazette* (for January 7, 1854) is a notice of a case, reported to the Pathological Society of London, of a man who passed, at various times, thirty-one gall-stones from a fistulous opening at the umbilicus; five years after the last one passed, he died of some other disease, when the gall-bladder was found almost obliterated.

Biliary calculi may be situated in the substance of the liver, in the biliary ducts, or, as most commonly, in the gall-bladder. Cruveilhier speaks of unilocular and multilocular cysts formed by dilatations of the smaller biliary ducts, and containing calculi. Abscess of the liver may open externally, giving rise to a fistulous opening, very difficult to close; calculi are sometimes passed from such fistulæ, of which Van der Wiel (*Obs. rares de Méd.* tom. i. p. 190, &c.) gives many curious examples. It may be questioned whether the first and second cases did not originate in a hydatid or other cyst, with which the gall-bladder became afterwards adherent, hence allowing the passage of the calculi through its opening. In the first case, the surgeon, and in the second, nature opened the tumour, which was followed by a quantity of yellow, watery

fluid, for four years in the former, and for thirteen months in the latter, before the passage of the calculus, which was immediately preceded by symptoms indicating a change in the character and seat of the lesion.

In the cases above cited, the gall-bladder was the seat of the calculi; the situation of the fistula near the umbilicus, in the first two and the last cases, may be owing either to hypertrophy of the liver, enlargement of the gall-bladder, or to displacement downwards by dress; in the third case, the fistula was in the right hypochondriac region, where the gall-bladder would be situated in a normal liver. The appearance of the fistula, in so many instances, at the umbilicus, far from the normal situation of the gall-bladder, would seem to indicate a connection between this course and the direction of the obliterated umbilical vein and its branches, forming the round cord of the liver—which would serve as a guide to any collection of serous, biliary, or purulent fluid tending to open externally. The secretion from the gall-bladder was considerably changed, becoming at times more like a troubled serous or mucous fluid than bile; in the last case, the coats were thickened and the viscus almost obliterated, as is generally the case when the gall-bladder is irritated by calculi. The ages of the individuals were 50, 55, and 77; and, in the third case, intemperate habits might be chargeable with the disposition to biliary trouble.

The symptoms preceding the opening of the fistula are not very constant; obscure pains and sense of uneasiness in region of liver; bilious vomitings and purgings; a swelling, more or less well defined; pains, resembling colic, caused probably by pressure on, or irritation of, branches of the hepatic plexus, quite different in intensity and duration, and also in seat, from the pains attending the passage of calculi through the biliary ducts; and immediately before the opening of the fistula, shivering, throbbing, and a sense of pricking on the passage of the calculus. There is not of necessity any jaundice, as the biliary ducts may be free, nor any trouble in the digestive, respiratory, cerebral, or urinary organs, and no ascites or indication of biliary obstructions.

The gall-bladder, irritated and inflamed by the contact of the calculi, contracts adhesions to the contiguous parts; ulceration establishes itself, and the tumour opens or is opened externally, or internally into the stomach, duodenum, or transverse colon. No. 565 in the above-mentioned Society's catalogue is a calculus which passed from the gall-bladder directly into the colon, and was discharged per anum; it measured 3¾ inches in its greatest, and 3 inches in its least circumference. Another case is alluded to, in which a calculus, nearly two inches in length, passed directly from the gall-bladder into the duodenum. In the first case, which was a female, 75 years of age, "a small, puckered cavity, about the size of an olive, being all that remained of the gall-bladder;" death took place, from scirrhous disease in the region of the gall-bladder, about three months after the passage of the calculus.

The *prognosis* in these cases does not seem unfavourable; in two of the cases the fistula healed up; in one the patient lived five years afterwards, with

the gall-bladder nearly obliterated, and died of some other disease; and in the other there was a lapse of at least seventeen months, without any aggravation of disease, as far as the fistula and calculus were concerned. Indeed, the circulation of the bile and the digestion seem to go on perfectly well, as Cruveilhier remarks, without the aid of the gall-bladder. The invertebrated animals, many fishes, birds, and mammals have no gall-bladder, and it is occasionally absent in the human subject. No. 552 of the same Society's catalogue shows a congenital deficiency of the gall-bladder, in a man 26 years of age, who died in the Massachusetts General Hospital, of pneumonia. No. 553 is a case of atrophy of the gall-bladder, from obliteration of the cystic duct, in a woman 80 years old, whose death did not seem to be caused by bilious trouble, to which she had been subject for three or four years.

The indications for *treatment* are very simple, consisting in the administration of narcotics to calm pain, and poultices and sponge-tents to soften and dilate the parts. When the tumour is much inflamed, and pain severe, surgeons advise puncture; incision was sometimes practised by the French, as if for an abscess, this mode being considered less dangerous than to risk the tumour opening into the abdominal cavity. If the tumour be interfered with at all, it would seem best to open it by means of caustic potash, by which means adhesive inflammation would be set up, and the peritoneal cavity be intact. Considering the difficulty of the diagnosis, and the fact that, as far as is known, the cases do well without interference, it seems the wisest course to allow the tumour to open itself. The resulting fistulæ do not seem to impair the constitution; and when the calculi, which keep them open, come away, they heal of their own accord.

In the *Mémoires de l'Académie Royale de Chirurgie*, tom. i. part 1, 1743, page 255, is the celebrated memoir, by the elder Petit, on "Tumours formed by the Retention of Bile in the Gall-bladder, often mistaken for Abscess of the Liver." Some cases are there given, bearing on the subject of this paper, which are here reproduced, that the symptoms and treatment of this rare affection may be more fully indicated in a single place. The cases are numbered in continuation of those already given.

Case V.—He was consulted in the case of a lady who had a tumour in the region of the liver, which was regarded as an abscess. The tumour was opened, contrary to his advice. Seven or eight months after, the lady consulted him for a fistula, from which flowed nearly pure bile. The fistula was dilated, and a calculus removed.

Case VI. shows that the presence of a calculus is not necessary for the production of a fistulous opening. A lady, 66 years old, subject to hepatic colics for many years, with retention of bile, had a tumour in the right hypochondrium; this became inflamed, suppurated, and opened externally, to the great relief of the patient. A fistula was the result, the secretions for a long time being a limpid fluid, and finally bile; the fistula closed and opened several times. The patient died, worn out by chronic disease, impaired nutrition, and fever. The gall-bladder was firmly adherent to the abdominal parietes.

Case VII.—A man, 45 years of age, somewhat emaciated, with a frequent dry cough and disordered digestion, was attacked with bilious disease; there was at first a moderately hard swelling, without pain, extending from the right hypochondrium to the middle of the epigastrium; urine very abundant, troubled, and red; stools clay-coloured. He underwent the usual treatment of aperients, bitters, plasters, &c., but finally lost his appetite and died. On examination, the gall-bladder was immensely enlarged, and adherent to the peritoneum and abdominal parietes; it was filled with a limpid, but viscid and bitter, liquid; more than sixty calculi were taken from it. This case shows the probable condition of things before the ulceration of the gall-bladder and the escape of the calculi through the fistulous opening.

Case VIII.—A woman, aged 66, for many years subject to hepatic colics, perceived on the right side of the abdomen a tumour, extending from the edge of the false ribs to the anterior superior spinous process of the ilium. When the tumour appeared, she was free from any biliary symptoms; so that she neglected it until violent colics came on, with fever. Her symptoms were supposed to be independent of the tumour, which appeared to be confined to the integuments; after being bled, she used tonics, with relief; but, for about three years, she was frequently troubled with the same symptoms, being either very constipated or with a bilious diarrhœa, with irregular attacks of fever. The tumour in the mean time grew painful; poultices were applied, suppuration took place, the tumour opened itself in about two months, and a slight discharge, during several months, almost dissipated the swelling. The fistulous opening then began to close and open alternately, with a slight swelling and pain at the edges. A limpid fluid escaped, and finally a sudden gush of bile, amounting to a quart in twelve days; the flow of bile gradually ceased, and the fistula closed up. In this way the fistula alternately opened and closed, furnishing at one time a serous and purulent fluid, and at another pure bile; the pain and fever were always relieved by the discharge of bile. Death took place from phthisis. On passing a sound in the fistula, it penetrated about five inches obliquely upward, towards the gall-bladder; the sound entered a kind of ligament, which attached the gall-bladder to the abdominal parietes, an inch and a half below the cartilages of the false ribs; this ligament was penetrated by a fistulous canal leading from the gall-bladder to a sac secreting pus, situated between the two oblique muscles, and which emptied itself by the external fistula. The gall-bladder had several elongated cul-de-sacs, in which calculi were lodged.

Case IX.—A woman, 39 years old, after suffering great and almost constant colic pains in the epigastric region (most acute under the ensiform cartilage) for two months, during which she was treated by antiphlogistics and narcotics, perceived a tumour in the region of the gall-bladder. This gradually increased in size, following the linea alba, on the right side, to within an inch of the umbilicus; it opened spontaneously, about six months after its appearance. About four ounces of a purulent matter flowed out, in which floated five or six calculi, of the size of a pea, of spongy texture, light, easily crushed, and inflammable; the suppuration was very abundant for two months. In the course of six months, seven or eight stones like the above were passed; the more free the discharge, the less were the pain and weakness. A probe passed about four inches before reaching the gall-bladder, and caused the discharge of bile accompanied by another calculus. The sinus, being very tortuous, was incised, when bile was seen issuing from a hole in the middle of the rectus muscle, about three inches above the fistulous opening; through this hole the

sound passed into the gall-bladder without obstacle; the bile and other discharges passed freely through this opening for two years, during which the hole in the rectus muscle formed adhesions with the skin, causing a secondary fistula. The patient enjoyed good health, and was quite free from pain and weakness. In case of obstruction by a calculus, it would be very easy to dilate the opening in the rectus by prepared sponge or other means, and calculi could be extracted as from the urinary bladder.

Case X.—A woman, aged 74, had a colic, followed by universal jaundice; bleedings, deobstruents, and aperients relieved the symptoms, but a tumour appeared in the right hypochondrium, which suppurated, leaving a fistulous opening; this closed from time to time, causing intense suffering. For a year the tumour was considerable, rather nearer the vertebral column than the middle of the hypochondrium; the fistula opened near, and a little below, the umbilicus. On one occasion of its closure, a probe was forced in, causing a discharge, followed by great relief; in the course of the fistula, a calculus was felt; an incision enabled the surgeon to seize and extract a very adherent calculus, four inches long and three inches in circumference, smooth at one end, but having at the other several small cavities, in which were lodged fleshy masses, the cause of the adherence. Another opening was also found, leading to the opposite side, quite into the left hypochondrium, where there was no appearance of tumour, which nevertheless contained a calculus, which was removed also by incision. The patient was completely cured at the end of two months.

In volume ix. of the same *Mémoires*, for 1757, p. 107, are two cases, similar to the above, which will complete the list to be quoted here. Many more might be adduced, but without rendering the subject clearer.

Case XI.—A lady (age not given) had a tumour of the gall-bladder, the integument being red and inflamed; it was opened as an external abscess, and clear pus was evacuated. Some time after this, bile was discharged through the opening. It was evidently an ordinary abscess, in which the gall-bladder was implicated; the adhesions naturally formed were sufficient to prevent the bile from entering the abdominal cavity. By incision, a calculus was extracted from the gall-bladder, of the size of the largest acorn. A fistula remained for many years. The patient died at an advanced age, enjoying good health to the last.

Case XII.—A man (age unknown) had a fluctuating tumour above the umbilicus; it was opened, and a very fetid pus discharged; on probing the wound, a few days after, a sinuous canal, two inches long, was found, at the bottom of which a hard body was felt, apparently movable; by a slight dilatation, a calculus of the size of the thumb was removed by forceps; two smaller ones afterwards escaped; a fistulous opening remained, from which there was a continual oozing of biliary matter.

The two latter cases show a difficulty in the diagnosis, or rather the coexistence of abscess of the liver with a tumour of the gall-bladder. The abscesses were properly opened by the surgeon; the escape of the biliary matter and calculi being the consequence of the action of the pus on the gall-bladder.

Remarks.—The principal question resulting from the study of these cases is, how to distinguish these tumours of the gall-bladder from an abscess of the liver. The differential diagnosis is not always easy, and mistakes have been

made by surgeons, instances of which Petit enumerates; the gall-bladder has been opened, instead of a supposed abscess, and patients have died from abscess of the liver, from fear of opening a supposed tumour of the gall-bladder. The difficulty is the greater that the preliminary symptoms in both cases may be the same, or the absence of the ordinary phenomena may mislead the surgeon as to the gravity of the disease; both may commence by a painful swelling in the region of the liver, accompanied by jaundice, clay-coloured stools, high-coloured urine, great thirst, bitterness in the mouth, intense itching, uneasy sleep, fever, vomiting, and other symptoms of retention or obstruction to the passage of the bile. If suppuration takes places, the pain and fever diminish, irregular shiverings come on, and a tumour appears, soft and fluctuating; these would be the usual symptoms of abscess; but an accumulation of bile in the gall-bladder may present the same swelling, fluctuation, shiverings, and diminution of pain and fever after the acute stage has passed. The pain, however, of the abscess is generally pulsative or throbbing, which is not the case with tumours of the gall-bladder; in the former, the constitutional trouble is great; in the latter, trifling. The tumour of the abscess is diffused, and the integument over it œdematous; that of the gall-bladder, distinctly circumscribed, rarely accompanied by œdema of the integuments. The abscess may occupy any part of the hypochondriac and epigastric region; the tumour of the gall-bladder is below the false ribs, and beneath the rectus muscle. The fluctuation in abscess is not apparent till long after the appearance of the swelling; is not always easy to ascertain; is first noticed at the centre, and gradually extends to the circumference, the latter having a hard and swollen edge. The fluctuation in tumour of the gall-bladder appears from the moment of the swelling, and suddenly; is easily recognized, as well at the circumference as at the centre; and is generally free from surrounding hardness.

These differential points, laid down by the elder Petit, 120 years ago, would seem sufficient to distinguish between abscess of the liver (a dangerous disease) and tumour of the gall-bladder (a comparatively trifling affection), the former requiring active interference on the part of the surgeon, while the latter may generally be left with safety to take its natural course.

November, 1854.

ART. V.—*On the Use of Oil of Valerian in the Typhoid Disease.* By J. LEASURE, M. D., of New Castle, Laurence County, Pa.

ABOUT the middle of September last, we were visited by a rather mild epidemic dysentery, which did not spread with any alarming rapidity, nor was it attended by any unusual symptoms, except that there was a much greater discharge of pure blood than we had been accustomed to meet for some years in our epidemic dysenteries.

About the same time we had a few cases of our usual autumnal remittent fever, and also a number of cases of masked intermittent fever or "dumb ague," as it is familiarly called in many portions of the Western States.

Towards the latter end of the month, all these forms of disease seemed almost insensibly to pass into the abdominal typhoid disease, familiarly known in some localities as typhoid fever, and which is frequently confounded with a low form of the regular remittent fever of this and some of the adjoining malarious districts, and which is also called typhoid fever.

Though called by the same name popularly and sometimes professionally, it is hardly necessary to specify the distinctive differences between the two diseases; suffice it to premise that the disease which we propose to treat of in the following remarks, is that form of fever in which the principal lesion is in the mucous follicles of the alimentary canal and generally accompanied by a diarrhœa, always recognizable after having been once seen, and unfortunately only too familiar to many of our professional brethren, in districts where it is an annual but unwelcome visitor.

Case.—On the night of the 6th of October last, I attended Mrs. W., aged 26, in labour with her third child. Presentation natural, of the vertex, and labour over in an hour and a half. Had not felt quite well for a week previous, having had some dull pain in the forehead and lumbar region, but did not go to bed or take medicine. On visiting her the second morning after her confinement, on looking at her tongue, I found it presenting the same appearance as the tongues of some of my patients labouring under the typhoid disease. Still, she had no fever, nor any unusual tenderness over the abdomen, and I concluded to wait for further signs of mischief before commencing active medication, for what might only be a coincidence in appearance between the tongues of Mrs. W. and my typhoid patients. There were four or five cases of the fever next door, and I left orders that if there should be any diarrhœa, or other untoward symptoms, before my return, I should be immediately sent for.

I was not sent for, but, on my visit next morning, I found her labouring under an attack, having all the characteristics, of a very violent bloody dysentery, and which had commenced some hours previously. The pulse was 120; skin rather cool and moist, with an anxious, haggard expression of countenance indicative of deep abdominal distress. There was no unusual tenderness on pressure over the abdomen except a little soreness in the right iliac fossa; lochia continued natural. This state of affairs gradually changed until the case presented all the characteristics of typhoid fever as manifested in my other patients labouring under the legitimate disease. Still, the lochia continued normal in *quantity*, but very dark and fluid, like dirty water containing the washings of bloody meat. The discharges from the bowels were the dirty-brown dejections nearly resembling old beef-pickle, so common in my other cases, and the mental and physical signs altogether were of the most discouraging character. I supposed, of course, that the case was hopeless, as neither I, nor any of my professional friends whom I consulted, had ever known a recovery from an attack of the real abdominal typhoid disease in a lying-in woman. I attempted to moderate or control the excessive diarrhœa by a free use of opiates and such astringents as answered the purpose in the other cases. I gave oil of turpen-

tine till satisfied that it was of no avail. I gave nitrate of silver in the vain hope that it would accomplish something, but it seemed to have no observable effect even when given in doses of a grain and a half every hour for many consecutive hours. During all this time, up to the ninth day after confinement, there was milk in her breasts sufficient for her child, which I permitted to nurse from the opinion that it *might* have a salutary effect on the mother, and because I entertained no fear for the child, as no cases of the disease had as yet attacked young children. (The child was not injured and lives yet.)

At this period (the ninth day after confinement), milk ceased to form in the breasts, and the patient gradually grew weaker until the 25th, the nineteenth day after labour, when I found her to all appearance moribund.

The diarrhœa had continued at the rate of from five to eight discharges in every twenty-four hours, sometimes involuntary; and, about 2 o'clock on the morning of the 25th, she began to vomit. When I visited her at 8 o'clock in the morning, I found her vomiting a dark-brown substance resembling muddy coffee, which was ejected out of the mouth as out of a spout, and where it ran over her linen and that of the bed, it stained them a dirty-greenish colour mixed with brown.

Her extremities were entirely cold, pulseless, and bedewed with a clammy sweat; her eyes were sunken into the bottom of their sockets, and her lips and nails blue as I have witnessed in the collapsed stage of Asiatic cholera. Indeed, the whole appearance of the patient resembled a case of malignant cholera in the last stage. Believing the case hopeless I did nothing until 2 o'clock, P. M., when I visited her again accompanied by my friend, Dr. J. H. M. Peebles, who coincided with me in a most unfavourable prognosis, but suggested the use of small potions of hot brandy and artificial warmth to the extremities (mustard had been freely applied without any effect). At 9 o'clock at night, I visited her again and found her in *statu quo*.

The hot brandy was thrown up as fast as swallowed, and the extremities were as cold as before, notwithstanding the artificial heat which had been constantly applied by hot bricks and bottles of hot water. I felt loth to go away without at least seeming to *want* to do something, and some passing thought, I scarcely know what, suggested the use of small doses of the oil of valerian frequently repeated. Possibly, I thought it might stop the vomiting; or, peradventure, if retained in the stomach, it might stimulate the nervous power which seemed so greatly deficient in this as well as my other typhoid patients. I ordered one drop of the oil of valerian every hour, and that the artificial heat be continued to the extremities.

26*th. Morning.* Patient still living; has not vomited since 2 o'clock this morning, and but twice since she took the oil of valerian. Some warmth has returned to the extremities, and there is pulse at the wrist, small, quick, and one hundred and thirty-two strokes to the minute; no diarrhœa; medicine to be continued as before. *Evening.* Extremities quite warm; pulse 120; profuse warm perspiration with a most disagreeable odour; no vomiting since last visit; one voluntary discharge from the bowels, of same appearance as before; nails and lips becoming more natural in colour, and patient says she feels a little better; medicine as before.

27*th. Morning.* Patient rested quietly; took her drop of oil every hour; vomited none; mind seems clear; had two dejections, the last a little more consistent; still sweats profusely and same disagreeable odour from it, and the sweat stains the linen of a dingy yellow colour; says she feels better, and *is hungry.* To have a little beef-tea; medicine as before. *Evening.* Has not vomited; has had one discharge from the bowels, more consistent

than any former one; has taken half a pint of beef-tea; asks for wine; pulse 100. To take beef-tea and oil as before, and half a tablespoonful of port wine whenever she asks for it.

28*th. Morning.* Rested quietly; slept some between doses; no vomiting; one operation on the bowels almost natural in colour, and no longer watery; pulse 100; still sweats, and same disagreeable odour; warmth natural; feels stronger. To take one drop of oil of valerian every two hours; beef-tea and wine as before. *Evening.* Says she has had a "good day;" no vomiting or nausea; no operation on the bowels; does not sweat so much; pulse 98, soft, and tolerably full. To take the oil every three hours during the night; beef-tea and wine *ad libitum.*

From this time she gradually recovered. The oil was continued at longer intervals for a few days longer; the diet was cautiously made more solid and stimulating, and, on the 2d of November, the milk began to return in her breasts, and in a week from that time she dismissed her nurse and took charge of her child, and may now be said to have perfectly recovered.

During the progress of this case, and before I had used the oil of valerian, I had a patient, a fine young man, who died from the typhoid disease under symptoms of pure prostration, or perhaps more properly, from a want of recuperative vigour of the nervous system, which refused to respond to all the stimulants I addressed to it. This, indeed, seemed to be the principal difficulty to contend with in every instance, and that, too, from a very early period in the sickness.

The marked change which followed the use of the oil of valerian in the case of Mrs. W. led me to try it in other low cases of the same disease, though without being very sanguine of the same success; for, after all, nature, and not the remedy, might have deserved all the credit of her recovery. The other cases were not accompanied by the same extreme state of affairs as to vomiting, prostration, collapse, &c., but the same result followed, viz: the most profuse sweating, with the same odour, and lasting from one to four days, and, in every instance, followed by a rapid amelioration of all the disagreeable and dangerous symptoms.

I now felt encouraged to give the oil a trial in the early stage of the disease, and in doses of one or two drops, varying in time from two to four hours; it early induced a copious diaphoresis, speedily followed by a mitigation of the disease. After I commenced a general use of the oil, I rarely gave any medicine for the diarrhœa, which mostly subsided as soon as the sweating began, and if the bowels were entirely closed for a week or ten days, I rarely interfered, for I found the patients steadily improve with a constipated state of the bowels, and I was only too well satisfied to "let well enough alone." In two or three instances I gave a teaspoonful of castor-oil, with eight or ten drops of oil of turpentine, where I thought an aperient indicated. I have, in all, treated twenty-two cases as above, some of them complicated by pneumonia with profuse bloody expectoration, and in which I did not find it necessary to make any addition to the treatment with the oil of valerian.

Other cases I have treated without the oil, but they were my mildest cases, and did not progress so satisfactorily as the more grave cases treated by the oil alone.

One patient treated with the oil, after seeming to be convalescent for a week, suddenly died, with all the symptoms of perforation of the intestine.

Pari passu with the progress of these cases, I had a couple of cases of very violent delirium tremens, in which I wished to avoid the use of opiates and alcoholic stimulants, and used the oil of valerian in the same small doses frequently repeated, as in the typhoid disease, and, unquestionably, the delirium subsided while the patients took the remedy, and returned with violence as soon as it was suspended a few hours, to subside again as promptly under its resumption.

In my typhoid cases there was little or no delirium where I gave the oil, while it was a very troublesome symptom in *all* the cases in which I did *not* use it, and one which I have always found very annoying in my former experience with the disease in question.

I do not wish it to be supposed that I am an advocate for the indiscriminate use of the oil of valerian in all cases of the typhoid disease, because it has seemed to be useful in a limited number of cases in a particular epidemic, but I do think that it may prove a valuable agent in the treatment of certain low forms of the typhoid and kindred conditions, in which an agent is required to rouse up the prostrated energy of the nervous system, thereby enabling *Nature*, our only real *vis medicatrix*, to rally her vital forces to the expulsion of disease and the reparation of its ravages.

ART. VI.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon U. S. A. (Continued from p. 101.)

WE will proceed to consider the epidemic fever of the third era, from 1817 to the present time, and the first in order occurred in

1817. In relation to this epidemic we have ample evidence. Even Dr. Hume says: "All we can now gather is, that the first city case was in Lynch's Lane; how it got there, no one can say." He ought to say how it "got there," if it was imported directly from the West Indies, as seems to be intimated subsequently.

Dr. Shecut is good authority; he was an eye-witness of the disease, and an active practitioner of medicine.

"In this year it commenced in the south-east section of the city, at or near Lynch's Lane, and progressed northwardly and partly north-westwardly, to

the market, and along the lower end of Church Street; of which places it is to be remarked, and particularly of the two former, that they are on made lands, having been formerly creeks, which intersected the city. By the middle of August, its extension and fatality were greatly increased, and in the latter end of the month, it infested most parts of the city, except the north-west.

"All the causes of yellow fever existed—heat and moisture in excess; animal and vegetable putrefaction, in its usual quantity; sinks and drains, as heretofore; and, to crown the whole, there was a great deficiency of electricity."

Quantity of rain:—

	June.	July.	August.	September.	October.	Total.
Inches	8.90	5.20	9.35	5.45	4.75	33.65

"The greatest number of deaths that have been recorded in the city, from yellow fever, from the year 1748 to the present, a period of sixty-nine years, are those of the present year. Total number of deaths this season, 272. Great as this number may appear, if we notice the number of deaths from yellow fever in the years 1699, 1703, &c., it will be found that according to the increase of population the proportion is considerably less now than at any period of the 17th and 18th centuries."

Dr. Rush saw a letter from Isaac Norris to one of his correspondents, dated Charleston, Nov. 18, O. S., 1699, which says that "150 persons had died in Charleston in a few days; that "the survivors fled into the country;" and that "the town was thinned to a very few people"—facts which show that yellow fever was much worse soon after the settlement of the town than in 1817. It is hoped that Dr. Rush's facts are not so "flimsy and imaginative" as Dr. Hume considers his arguments.

No proof, nothing but assertion, has ever been offered that the yellow fever of 1817 was imported. It has been said by the advocates of contagion, Drs. Strobel and Hume, that commerce revived at the close of the last war with Great Britain, and introduced yellow fever; we should say, on the contrary, that commerce revived and brought subjects for the disease. These advocates of contagion seem to speak of 1817 as the first commercial year after the war; but they ought to recollect that peace was concluded early in 1815, and that this and the following year—neither of them epidemic—were more important, commercially, than 1817. We are indebted to Drs. Dawson and De Saussure, *Census of Charleston*, for the following:—

EXPORTS. *Value.*

Fiscal year.	Domestic produce.	Foreign produce.	Total.
1 Oct. 1814, to 30 Sept. 1815	6,574,783	100,346	6,675,129
" 1815, " 1816	10,446,213	403,196	10,849,409
" 1816, " 1817	9,944,343	428,270	10,372,613
" 1817, " 1818	11,184,298	256,664	11,440,962
" 1818, " 1819	8,014,598	236,192	8,250,790
" 1819, " 1820	8,690,539	192,401	8,882,940

IMPORTS. *Value.*

Fiscal year.	Duties collected on imports in Charleston.	EXPORTS AND IMPORTS OF THE U. STATES.	
		Exports.	Imports.
1 Oct. 1814, to 30 Sept. 1815	1,400,887	52,557,753	113,041,274
" 1815, " 1816	1,474,474	81,920,452	147,103,000
" 1816, " 1817	1,145,678	87,671,569	99,250,000
" 1817, " 1818	1,308,104	93,281,133	121,750,000
" 1818, " 1819	813,829	70,142,521	87,125,000
" 1819, " 1820	613,698	69,691,669	74,450,000

From these tables it is apparent that in the years 1815 and 1816 there was plenty of commerce, and plenty of opportunities for the importation of fever; and there were numerous subjects among the strangers and commercial men; but the epidemic causes did not act with intensity, and there was no malignant fever.

It clearly appears, that of the commercial years 1815, 1816, 1817, 1818, the year 1817 was the least commercial of all. The amount of duties on imports is the criterion of the commercial intercourse, the very intercourse which, according to the contagionists, should import yellow fever; and we find from the tables that less revenue was collected from imports in the port of Charleston, in 1817, than in either of the other great commercial years succeeding the war. The years 1814, 1815, 1816, and 1818, were among the most healthy of all years since the settlement of the town.

In Dr. Strobel's book, the following passage occurs (p. 200):—

"The yellow fever had not prevailed in Charleston from the year 1807 up to this time (1817), a period of ten years. Upon the revival of commerce, however, at the close of the war of 1812, a sudden irruption of yellow fever took place, at a time when the city was filled with strangers."

This passage is calculated to mislead. In the first place, there was yellow fever in Charleston in both 1809 and 1811, notwithstanding the embargo. Then, one might be led to suppose that the war lasted from 1812 to 1817, whereas the treaty of peace had been ratified by the United States and Great Britain several months more than two years before the epidemic broke out; that this was the first year of commercial intercourse, when it was the third; and that there had been no strangers in the city until this year, when they had been necessarily present for more than two years previous to the breaking out of the epidemic.

1818. Epidemic fever did not prevail, though one of the decidedly commercial years. Dr. Shecut says of this year:—

"In 1818 many of the causes of yellow fever existed; the heat and dryness in excess; animal and vegetable putrefaction in the same proportion; and sinks and drains remained as before; but there was no epidemic yellow fever, and only a very few sporadic and doubtful cases of it."

1819. This was an epidemic year, though not to the extent of 1817. Dr. Shecut says of this year:—

"The present season, to the 25th of August, has been hot and wet, and remarkable for the excessive quantity of thunder and lightning."

Again: "The prevailing diseases for the season were intermittent, remittent, and catarrhal fevers; and by the 10th of August, a few sporadic cases of yellow fever were detected in the city, the atmosphere of which gave every evidence of a disposition to generate an epidemic yellow fever therein; and the immense number of strangers in Charleston, some of whom, both in point of situation and mode of living were proper subjects of disease, became soon its victims.

"All the causes of yellow fever, with the exception of a deficient electricity, existed this season as heretofore; the heat, though not excessive, was great; and the quantity of rain such as to constitute a wet season."

It is evident that the "plague had begun" at the time Dr. Shecut was writing, but he gives no intimation that it was imported; on the contrary, we are distinctly told that by the 10th of August a few sporadic cases (what the doctor doubtless hoped were sporadic) of yellow fever were detected in the city; and he gives the reasons for apprehending a malignant epidemic—the state of the atmosphere, the heat, the wet season, the immense number of strangers and their imprudent manner of living, and the filth, for he says that every cause "except deficient electricity existed as heretofore." No mention is made of importation from any part of the world, and if the disease had been imported, Dr. Shecut would have told us so. But it was not imported; it was generated in the city, notwithstanding "that Charleston is beyond the norther limits of *self-generating* epidemic yellow fever."

Dr. Shecut believed that the electrical condition of the atmosphere has much agency in producing yellow fever, as in 1817; while in 1818 an *electrical equilibrium* was kept up during the whole summer by excessive thunder and lightning, and the city was healthy; 1819 was "remarkable for the excessive quantity of thunder and lightning," and Dr. Shecut hoped when his work went to press, which was before the end of the epidemic season, that the city would escape from the ravages of malignant fever. But in this he was disappointed; epidemic fever prevailed, and there were 172 deaths. Dr. Simons says that this fever was less general than that of 1817, "although the disease was equally violent in its type."

We see that Dr. Shecut ventured to predict, though not very positively, that the yellow fever would not become epidemic in 1819; and that he was disappointed. In the spring of 1853, Dr. Hume predicted, publicly in the newspapers, from meteorological observations, that yellow fever would prevail in Charleston during the summer, in consequence of which many persons left the city. But the summer of 1853 was notoriously one of the most healthy in a number of years. The condition of the streets was horrible. During the wet season of 1817, not a single street in Charleston was paved, and it was no uncommon thing for loaded drays to be *set* or *stalled* in East Bay street, so deep was the mud. A great era commenced in 1819, when East Bay and Elliott Streets were paved. Let the good work proceed—not a single street, lane, or alley ought to remain unpaved.

1820. The fever prevailed slightly, but did not become epidemic.

1824. The next epidemic fever after 1818 occurred in 1824, when, says Dr. H. Y. Simons,

"It again raged in violence equal to any period in the annals of this country. The first cases were exhibited in a narrow, dirty alley, occupied chiefly by debauched females and sailors; from thence it extended among the shipping, then generally attacking strangers, and latterly children. Individuals who had lived here many years, but had not spent a yellow fever year here, died of it. Several cases were known of persons being seized and dying, who had been here in 1819 and even 1817. Children, some as old as ten years of age, and one as old as fourteen, natives and inhabitants of Charleston, died with this disease; many negroes from the country were seized, but the disease was much milder and few deaths occurred among them.

"Charleston Neck, which adjoins the city, and which is all original land, was quite healthy; strangers residing there generally escaped; and in almost every instance where it did occur, the cause could be traced to exposure in the city."

Dr. Simons concludes:—

"1. The yellow fever is endemic of Charleston, and has its origin in causes there.

"2. All persons who have not spent a yellow fever year there are liable to the disease, and it is questionable if they are wholly exempt until they have had the disease.

"3. Children from the age of one year to twelve are susceptible of this disease, although not in an equal degree with strangers.

"4. Persons living in warm latitudes are liable to this disease, but in a less degree than those of cold latitudes.

"5. Persons born and raised in Charleston, but who have lived for some time in cold latitudes, are liable to the disease.

"6. The disease is most prevalent and general in places most thickly populated, and where the earth has been made, although no place in the city is exempt.

"7. Charleston Neck, where the soil is original, is greatly exempt, although it would be impossible to say wholly so.

"8. From cases which have occurred, it appears very questionable if the disease cannot be twice taken; of this, however, it is impossible to be accurate.

"Cleanliness is all important, and could our streets be all paved, and the marsh and low land be filled up with some other materials than the scavenger's offal now used, we might hope for better things."

Mortality, 236: adult males, 160; adult females, 32; children under 12 years, males, 17; females, 27.

"It must be kept in mind that some died in the suburbs of the city, and very many on Sullivan's Island, which, if added to the list of 1824, would increase it considerably, and, I presume, those of preceding years."

"The spring of 1824 was uncommonly hot, many persons were sun-struck. Cholera infantum was very prevalent, and many children died of it. The atmosphere was close and dense throughout the summer. The bilious fever among the inhabitants when it did occur, was very violent in its type. At the termination of the yellow fever, catarrhal fevers, sore-throat, &c., supervened." *Carolina Jour. Med.* for January, 1825, pp. 1–20.

From Dr. Simons we learn, 1. That the fever of 1824 was not imported, but originated in the city. 2. That it extended from the point of origin to the shipping, and then among strangers generally. 3. That it was malignant, attacking children and negroes. 4. The season was very hot. 5. The sanitary police was not good. 6. Yellow fever is endemic of Charleston, and

has its origin in causes there. 7. The disease is most prevalent and general in places most thickly populated, and where the soil has been made. 8. Charleston Neck, where the soil is original, is generally exempt. 9. 1824 was an epidemic year.

There is not a particle of evidence that this fever was imported, and there is no doubt of its domestic origin. We have, then, three severe epidemics in succession, in 1817, 1819 and 1824, all of local origin, for not a shadow of proof has ever been offered that any of them were imported, against the positive testimony of Drs. Shecut and Simons.

1827. Yellow fever prevailed to such an extent that it may almost be considered epidemic. Dr. Simons says there were 67 deaths.

In August of this year, the disease was imported from Charleston to Fort Johnson, as stated in Dr. Selby's letter. Three soldiers of that post remained a night in the city; on the 9th day after, they were attacked with yellow fever; they were sick in company quarters;" ten or twelve men slept in the same room with them who were alike strangers to the climate; and yet there were no other cases of the disease in the garrison."

Here is an instance of genuine importation, and under the most favourable circumstances, the disease did not spread. The disease, which will be noticed in another place, also prevailed at Fort Moultrie. But how came the yellow fever in Charleston? No one has attempted to prove importation from the West Indies, nor can I find any account of the diseases of this year in Dr. Strobel's book, nor in Dr. Hume's paper.

1828. Dr. Simons states that there were 26 deaths in the city. The dengue prevailed as an epidemic.

1830. There were 29 deaths from yellow fever. Dr. Simons.

1831. This is not put down as a year in which yellow fever occurred in Charleston, but Dr. Selby gives some account of the fever at Castle Pinckney. "One of the Irish labourers died in the city of supposed yellow fever. Several other cases of fever occurred afterwards among the labourers," which Dr. Selby thinks were cases of bilious remittent fever.

1832. The information for this year is derived from Dr. Selby's letter to Dr. Strobel.

"The disease appeared in the city of Charleston, and from two or three of the first cases being traced to the Irish labourers at the Castle, it was supposed to have originated there. A deputation of medical gentlemen from Charleston visited the post, who thought the disease was caused by a 'quantity of shells which had been used for the purpose of filling up the parade ground;' and others thought the disease originated from the 'opening of two privies in the north wall, which had been bricked up for a number of years—their contents in an undecomposed state taken out in the month of August, and exposed to the action of the sun on the bank near by, together with the exposure of the lower story of the quarters to the action of the atmosphere, it being necessary to rip up the floor which had sunk down to the surface of the ground, and had remained in that state for some years.'"

Whether the disease arose from the shells on the parade, or from the

privies and the ripping up of the old floors, or both, it is evident that the disease was of local origin.

1834. Dr. Simons says there were 46 deaths in Charleston from yellow fever. Severe yellow fever prevailed at Castle Pinckney, and there were several deaths, among others Capt. H. W. Griswold, 1st artillery, who died October 23d. We have 46 deaths in Charleston and several at Castle Pinckney, showing an epidemic tendency, or a real epidemic. How came there so much yellow fever in Charleston and at the Castle during this year? We have no account of importation from the West Indies. It has never been pretended that the fever of this year was imported, and the cases and deaths must come under Dr. Hume's clause—"Charleston does occasionally produce sporadic cases of fever of domestic origin, but they do not extend, nor infect localities." Whether they extend or not, or infect localities or not, there was quite a sprinkling of yellow fever, of domestic origin, in and around Charleston. To the patient and his friends it makes but little difference whether such cases are sporadic, or really epidemic, whether of domestic origin, or of West Indian importation. To the patient, if fatal, the result is the same. There can be no doubt of the domestic origin of the fever of this year.

It will be noticed that Castle Pinckney was used as a Lazaretto previous to 1832. It was again recommended to the city council by their committee, December 20, 1853, for this purpose, and was on the point of being turned over by the Government, when some one had the good sense to put a stop to the project. A worse place for sick people could not well be selected. It is surrounded by marshes; in winter it is bleak and disagreeable; in summer, damp, unwholesome and unhealthy.

1835. Dr. Simons says there were 26 deaths.

From 1807 to 1835, a period of 28 years, we find 10 years in which yellow fever prevailed, more or less, sometimes severely, and at other times lightly; but in not one of these years have the contagionists attempted to point out a single instance of importation.

Dr. Strobel says: "In 1807, the embargo was laid, and instantly the yellow fever ceases, and we hear no more of it for 10 years. The embargo and non-intercourse laws had the effect of closing our ports from the year 1807 until 1812, when the war was declared, and lasted until 1815. Upon the declaration of peace, our commerce again revived, and in 1817, we had a return of yellow fever, and from that time to the present, it has occasionally occurred." p. 221.

It is not essential to our present purpose to have it determined whether the embargo and non-intercourse laws applied only to British commerce, or whether there was an embargo on all vessels coming from the Spanish, French and Danish West Indies; and there certainly was none on vessels from New Orleans; but we will take Dr. Strobel's passage as it stands, and inquire how yellow fever came in Charleston in 1809, for there was no foreign commerce, and there could be no importation; nor could there have been any in 1811, when yellow fever prevailed; nor in 1812, which Dr. Hume says was one of the years of epidemic visitation. Intercourse with

New Orleans was uninterrupted during the embargo; and the truth is, that the Port of Charleston has not been shut against commerce and the importation of disease in any year, from 1792 to the present day, except 1812, '13, and '14.

1838. We come to the first epidemic yellow fever to which the contagionists undertake to give an origin by importation, except by general and sweeping assertions. Dr. Strobel knew that there were severe epidemics in 1817, 1819, and 1824, as well as previously; but he makes no attempt to account for their appearance, except in a general way and by strong assertions. Dr. Hume merely mentions them, and passes them by in the same way.

The epidemic of 1838 is thought by many to have been the most severe ever experienced in Charleston, the number of deaths amounting to 353. In relation to this fever, Dr. Dawson and Dr. Saussure say:—

"It is well known that in the spring of 1838 occurred the dreadful fire, which laid nearly a fifth part of the city in ruins, and exposed to the action of the sun and air nearly 150 acres, saturated with the accumulated off-scourings of nearly six hundred families, leaving cesspools, wells, and cellars bare, and ready to give off their fatal emanations as soon as acted upon by the rays of the summer sun. It was not to be wondered at, therefore, that an epidemic arose which exceeded in severity all that had been known heretofore in Charleston."—*Census of Charleston*, p. 202.

Dr. Strobel gives a short account of this fever, which does not differ materially from that of Dr. Hume, and we will give the history, with the remarks of Dr. Simons, from the *Chas. Med. Jour.* for May, 1854, p. 345.

"In relation to the fever of 1838, Dr. Hume says: that the Lord Glenelg arrived at Boyce's wharf on the 4th of July, with yellow fever on board; that the Medora lay at Magwood's wharf, one hundred and twenty yards from the Lord Glenelg; and that the first case of yellow fever was the wife of the steward of the Medora, who frequently visited her husband in that ship; but it is important to add, that her case did not occur until one month after the arrival of the Lord Glenelg."

"In the first place, we deny that the Lord Glenelg had any yellow fever on board, and denied it at the time. And next, we ask, is it reasonable or feasible, or in accordance with experience and observation, that the disease should have been taken by the wife of the steward of the Medora, one month after the arrival of the Lord Glenelg, she not visiting this vessel, the infection remaining dormant in her system, and attacking no one else, not even those who were loading and unloading the said-to-be infected vessel, but reserving all its virulence for this poor woman, whose nearest approach to it was at the distance of one hundred and twenty yards, and that only occasionally."

"But in his anxiety to trace the disease to a foreign source, Dr. Hume forgets other more potent causes; for he says nothing of the extensive conflagration which occurred in Charleston, in the spring of that year, destroying nearly one-third of the city, nor of the effects of the consequent exposure of a large mass of heterogeneous matter to the influence of the sun and rain, nor of the number of strangers who came to the city for the purpose of rebuilding the dwellings which had been consumed by the fire, crowded together in boarding-houses deficient in number, and which strangers, already strongly predisposed to the disease, were the principal victims."

Dr. Joseph Johnson says:—

"On this principle, of a large surface exposed to putrefaction, may be explained the fact, that, after each extensive fire, have succeeded years of yellow

fever, desolating in proportion to the surface laid bare by the ravages of those fires respectively. (Dr. Johnson goes into an examination of the consequences of the great fires of 1796 and 1810, already cited, and then proceeds.) So also with other years, particularly the last and most destructive of fires in April, 1838, by which 25 acres of land, in the centre of the city, was laid open to the lodgement of water and exhalation of deleterious vapours from animal and vegetable substances in a state of putrefaction. The fire deprived eight or ten thousands of the inhabitants of their habitations. Many left the city, never to return. Strangers coming in as labourers, and residents, were attacked with yellow fever, and the desolation was awful. Charleston has not yet recovered from these unfortunate visitations."

"A similar disease followed the destructive fires in Savannah, Augusta, and, we believe, Wilmington, N. C."—*Chas. Med. Jour.* for March, 1849, pp. 57–58.

Dr. Strobel states, that:—

"On reference to the books of the Marine Hospital, we find the first case reported for 1838, to be that of Ryan, who was a seaman on board the Medora. He came in on the 4th of August, a decided case, which terminated fatally. He had been sick three days before his admission." p. 124.

Ryan was therefore taken sick on or about the first day of August, the same day on which Mrs. Martin, the steward's wife, was taken, and being the first yellow fever patient admitted into the Marine Hospital this year; it looks, coupled with Dr. Simons' testimony, that there was no yellow fever on board the Lord Glenelg, as if the vicinity of the Medora was one of the points of origin. We have no idea that the steward's wife took the fever on board the Medora from occasionally, or even frequently, visiting her husband; but if she did, what has that to do with the Lord Glenelg and importation? To suppose that she contracted the fever by contagion from the Lord Glenelg, is, to say the least, to suppose an improbability. We have no doubt that Mrs. Martin was indebted to her own residence for the fever, for Dr. Strobel says: that she "took sick on East Bay, opposite to Hard Alley," a most filthy part of the city. We have no proof whatever, so far, that the fever of 1838 was imported.

1839. Dr. Strobel says:—

"On the 7th of June, 1839, the first cases of yellow fever which occurred in Charleston, were brought to the Marine Hospital. They came from on board the ship Burmah, Webster. That vessel left Havana on the 1st of June." Again: "All of a sudden, however, on the 7th day of June, three cases of yellow fever were introduced into the city by the ship Burmah, from Havana, and then we learn for the first time, that the disease had been prevalent in that port as well as in Matanzas, as early as May. In ten days after the arrival of the Burmah, 3 cases are presented by the ship Leonore, and 1 by the ship Chatham. These vessels were both from Boston, that port as well as their crews being perfectly healthy at the time of their departure. They are officially inspected, and pronounced perfectly clean and healthy, so much so, that they were released from the quarantine which had been imposed. The Leonore lay in the stream the whole time from her arrival, and one patient, Simondson, never landed in Charleston."

It will be noted that Dr. Strobel says the fever was imported into the city from the Burmah, on the 7th of June. He must, therefore, refer to Cobb and Shute, who were carried to the Marine Hospital "late in the evening," and who died there, for the Burmah (Dr. S. himself says) lay in the stream

until the 22d of June, when she hauled to the Commercial wharves, where she lay until the 4th of July; yet we do not find the fever extending in and around that hospital, but we are always referred to the shipping.

Dr. Strobel goes on to say:—

"On the 17th of June, Ryder, Thorne, and Schmidt were brought in from on board the Leonore. Ryder and Thorne came in on the 4th day of their disease, and both died with black vomit. Schmidt, who came in on the 2d day, was discharged cured. The ship Leonore arrived in harbour on the 7th day of June, after a passage of 11 days from Boston. She lay in the stream all the time, and Schmidt, one of the patients, never landed in the city of Charleston until brought ashore sick. The Leonore lay in the stream 200 yards from the shore, and opposite to Central wharf. The ships Burmah, Medora, and Copia, from Havana and Matanzas, also lay in the stream during the same period.

Dr. Strobel admits that no communication between the Burmah and Leonore could be traced, but supposes it, without any reason, in our humble opinion. He omits, however, a very important circumstance. He states distinctly, that the Leonore lay in the stream 200 yards from the shore, opposite Central wharf, which is nearly up to Market street; and that the Burmah also lay in the stream during the same period. He does not inform us whether the Burmah lay only half a cable's length from the Leonore, or half a mile, a material circumstance; but it so happens, that others tell us that the Burmah lay off Roper's wharf, by the South Battery, and consequently, the two vessels were nearly half a mile apart. And now, will any reasonable person affirm that the Leonore could be infected by the Burmah, at the distance of half a mile in the stream? But let us refer to Dr. Simons.

"On the 7th of June, 1839, three patients were admitted into the Marine Hospital from the ship Burmah, which had arrived from Havana, of which I was informed by the physician of that institution. * * The remainder of the seamen on board who were sick, were sent to the Lazaretto, and the ship was thoroughly cleaned and ventilated, she being in ballast. She was in the stream and did not come to the wharf for some weeks after, and had no communication, as far as could be ascertained, with other vessels. On the 17th and 19th, cases were admitted into the Hospital from the ships Chatham, Leonore and Elizabeth Bruce. The Chatham and Elizabeth Bruce were lying at Fitsimmons' wharf (foot of Market street), the Leonore was lying in the stream opposite these vessels, and had never been to the wharf; the Burmah was lying in the stream off Roper's wharf; the distance between each of the vessels was therefore considerable, and there were a good many vessels between, on board of which no sickness as yet occurred. The Chatham arrived here from Boston on the 5th of June, in ballast; the Elizabeth Bruce arrived in Charleston from New York, 7th of June, in ballast; and the Leonore sailed from Boston, and arrived on the 7th June, in ballast; all the crews were well. Subsequently, the disease occurred in different vessels in the harbour, which it would be unnecessary to detail."

"The fever having occurred so early in the season and so soon after its appearance on board the Burmah, created a suspicion of contagion in the minds of some, but I could not, upon the minutest investigation, come to that conclusion; and a committee, consisting of Drs. Lopez, Moultrie, Geddings, Campbell, Winthrop and Horlbeck, the President, which was appointed by the Medical Society, after making a minute and thorough investigation, came to the conclusion, that the fever was not introduced by the Burmah, or by contagion, but was produced by the peculiar condition of our atmosphere. In other words, it was epidemic, and arose from causes among us. From that report I select the following:"

"By an examination instituted through reference to the captains and mates of the various vessels, whose information was given from the log-books, your committee have ascertained that they had, at the time of their sailing from the different ports, viz: Liverpool, Boston, and New York, for this port, perfectly healthy crews, with sound cargoes, incapable from their character of generating foul air. That there was no malignant disease at the time of their departure; that their crews had not, while in this port, any communication, either direct or indirect, with the Burmah or her crew; and finally, that *that ship*, in all these instances, lay in the stream, from a quarter to half a mile from them, except for a short period, of which mention will be made hereafter."

Again: "The presumption is thus fairly induced, that the cleansing and ventilation must have disinfected her (the Burmah) sufficiently of her foul atmosphere for purposes of safety, *else, why did none other of her crew remaining on board thence to the time of departure from our port, contract the disease?*"

"Again: 'Thus far your committee have satisfied themselves, that the transmission of the fever through the agency of the Burmah, is neither tenable as a fact, nor in accordance with the opinions of a great majority of the medical profession in this country.'"

"And again: 'Your committee, therefore, are of opinion, that the yellow fever which has prevailed, and still continues this season, has its origin, not from contagion derivable from those cases imported in the ship on the 6th of June last, but from local and general causes.'"

"I may here also remark," says Dr. Simons, "that in 1838, a fire occurred which destroyed one-third of the city of Charleston, and in this year and 1839, the exposure of an immense mass of materials on the surface of so much uncovered earth, and water in the cellars, were exposed to solar influence, sufficient of themselves to generate yellow fever."

"It must be here remarked, that the month of June was uncommonly hot; and the whole of this summer was remarkable for the great drought, as well as high temperature."

The remainder of Dr. Simons' report is devoted to a consideration of the importance of a full supply of pure and wholesome water, the planting of trees, of awnings to the vessels in the docks, the evils of small houses on filthy and partially filled lots, the disposal of the city offal in a more judicious manner than to use it for filling up low streets and marsh lots, the cleansing of the docks at a proper season, the preventing of exhalations from the drains, and clearing them out in the winter season, keeping the streets and yards clean, draining and filling up low lots, properly ventilating and keeping the cellars dry, prohibiting any more cellars, and burying the dead beyond the precincts of the city. Report on the yellow fever of 1839: also *Chas. Med. Jour.* for Novemb., 1851.

Dr. Simons' account renders it certain that the cases of yellow fever on board the Chatham, Elizabeth Bruce and Leonore, were not contracted from the Burmah, and the disease was doubtless of local origin, which is not surprising, considering all the circumstances of the city. There were also other cases of this fever in the harbour, with which the Burmah could have had nothing to do. Let us look at a portion of Dr. Lebby's letter.

"Capt. R., of the U. S. Lighter Valiant, lying in the cove of Sullivan's Island," was taken with this fever on the 16th June. "This man had not been in the city, from all the information I could obtain, for twelve days, at least, previous to his attack." He had black vomit and died.

This man, then, had not been in the city since the 4th of June, which was before the Burmah arrived in the stream, and he was taken sick before she went to any wharf; for Dr. Strobel distinctly informs us, that the Burmah was not hauled to the wharves until the 22d June, but "continued to lay in the stream." From circumstances, there is no doubt of this man having taken the disease on board of his own vessel.

The next case admitted was on the 20th June, from the lighter Sea Flower, the captain lying at the wharf at Fort Johnson. "This vessel had been engaged for some time in transporting materials from this port to Fort Moultrie—the captain had not been to the city for many weeks." This case must have originated at Fort Johnson, Sullivan's Island, or on board the vessel—certainly not on the Burmah.

"The third case was admitted on the 22d June." This person, a northerner, in high health, temperate, took charge of the Valiant as soon as R—— was taken sick. "This case was well-marked yellow fever from the commencement." He had been frequently to and from the city. On the next day a third case from the Valiant was brought in—a black man with bilious congestive fever;" and on that evening another black with the same fever, from the lighter Fame from Charleston." Immediately after the admission of the third case, Dr. Lebby suspected the Valiant, and suggested the removal of the crew, and to have her "ventilated and well cleansed."

"Capt. Harvey was placed in command of her, who stated to me that he found a large quantity of decomposed cabbages and potatoes, and other vegetable matter in a putrid state in her lockers, and a quantity of dirt and filth between her ceiling. Several of the negroes engaged in cleaning her were taken sick with intermittent and congestive fevers. She was scuttled and allowed to remain full of water for some days. After this she was pumped out, and continued healthy the rest of the season."

She continued healthy the rest of the season, just as the Burmah remained healthy after a thorough cleansing.

We have three cases of yellow fever, two cases of congestive fever in blacks, and several cases of intermittent and congestive fever among the negro labourers about the Valiant, not one of whom can be suspected of communicating with the Burmah. These three cases of yellow fever are about as strong instances of the "self-generating" process as have ever been placed on record. The lighters did not communicate with the vessels at quarantine, for Dr. Strobel inquired if "they did not communicate with vessels from Havana or Matanzas, lying at the quarantine ground." Dr. Lebby's reply is what follows:—

"I have taken considerable trouble to ascertain this fact, and I have no hesitation in asserting that, with one exception, there never was any communication with the quarantine vessels and the government lighters, nor with any one in the service of the United States. The exception alluded to, was the captain of the steamer Sumpter; on one occasion he sent his boat on board the John C. Calhoun, some days after she had been boarded by Dr. Simons. No ill effects resulted from it, for neither of the negroes that were in the boat were sick afterwards. The steamboat continued to perform her regular duties between the

city and the fort throughout the season, and though her engineer and mate were entire strangers, there were no cases of fever on board."

These extracts settle the question. There seems to be no doubt of the local or domestic origin of the fever on board of the Chatham, Elizabeth Bruce, Leonore, and the lighters in Charleston harbour; and it is certain that the disease was not contracted from the Burmah. Every one of Dr. Lebby's three cases of yellow fever was taken before the Burmah hauled to the wharf on the 22d of June.

Neither Dr. Strobel nor Dr. Hume says one word in relation to the condition of Charleston in 1839. Drs. Dawson and De Saussure say:—

"During the succeeding summer (1839) there still remained sufficient putrescent material exposed in the burnt district, to give rise to a second but less extensive epidemic."

In 1839 yellow fever prevailed in Augusta, Ga., and Dr. F. M. Robertson, now of Charleston, as chairman of a committee "to inquire into the origin and causes which gave rise to the late epidemic," addressed Dr. Geddings in relation to the Charleston fever of the same year. The questions and replies are as follows:—

"1. Do you consider the fever commonly called *yellow fever*, or what is termed *stranger's fever* in your city, a contagious disease?"

"*Ans.* In the whole course of my observation, in several epidemic visitations of yellow, or stranger's fever in this city, I have not witnessed a single fact favourable to the doctrine of contagion. On the contrary, facts, almost without number, have fallen under my observation, all producing the fullest conviction in my mind, that the disease is not contagious."

"2. Do you believe that a state of the atmosphere can be produced, capable of rendering the disease epidemic in a city, by the simple introduction of cases from other cities?"

"*Ans.* In reply to this query, I must likewise state, that I have never witnessed a single fact calculated to favour the belief that the disease can be rendered epidemic in a city, by the introduction of cases from other cities. Such an effect I hold to be impossible in any other way, than by the agency of either common causes or contagion. Believing, therefore, that contagion does not exist, my conviction is that when the disease prevails as an epidemic, it owes this character entirely to common causes, operating through the medium of the atmosphere of the place. A ship arriving in port, having yellow fever on board, may impart that disease to persons exposed to its atmosphere; but the disease produced under such circumstances, will never spread through the community, or in other words, assume the form of an epidemic."

"3. Do you consider the disease as it prevailed in your city, during the past summer, to have exhibited, in any degree, a contagious nature?"

"*Ans.* I have never, either in the epidemic of the past summer or of those which preceded it, observed a single fact or circumstance favourable to the belief in any contagious property. On the contrary, I have witnessed the most free and unlimited intercourse between the sick, and those who might be considered subjects for the disease, without the latter being attacked."

Here we have the whole subject. Dr. Geddings had witnessed several epidemics in Charleston, particularly those of 1838 and 1839; he was a member of the Committee which investigated the cause and origin of yellow fever in the latter year; he knew every circumstance in relation to the Burmah; and here we have his testimony. The whole subject—contagion, contingent

contagion, and non-contagion—is here presented. It is easy to understand that a disease is contagious, and that transmissibility means contagion, but it is difficult to understand the refinement of contingent contagion; and it appears to us that a disease must be either contagious or non-contagious, for we have no instance of contingent contagion in any of the numerous ills that flesh is heir to, particularly in smallpox, measles, scarlatina, &c.; and what the contagionists term so is, in our humble opinion, quite as liable to be extended to the shipping as from it. We have several instances of this. In 1817 yellow fever originated, was engendered, in Lynch's Lane, and extended to the shipping; and in 1824 Dr. Simons tells us that the fever was first "exhibited in a narrow, dirty alley, from thence it extended to the shipping," and then generally. In 1838 we have another instance in the Medora, in the case of Ryan, already alluded to; and in 1839 we have indisputable instances in the Chatham, Elizabeth Bruce, and Leonore. In 1849 we have the Numa; in 1852 the Clara Bell, with Capt. Cale's lady; and in 1853 we have an instructive example of this kind of contagion in the Barkelew. The shipping have been more "sinned against than sinning."

ART. VII.—*Synopsis of Thirty Cases of Ovariotomy occurring in the Practice of the Author.* By WASHINGTON L. ATLEE, M. D., of Philadelphia.

CASE I.—The first operation was performed on the 29th of March, 1844; patient, Mrs. G. S., aged 61 years. The incision extended from two inches above the umbilicus to the pubis; the tumour cystiform, bilocular, non-adherent, weighing about twenty-five pounds. The colon was involved in a broad pedicle. Both ovaries diseased. Constitution very feeble. *Death* from insidious peritonitis on the sixth day. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. viii. p. 43.)

CASE II.—Operation, 28th of August, 1844; patient, Miss L. P., aged 24 years. Incision from umbilicus to pubis; tumour extra-uterine, fibrous, weighing nearly two pounds. Pedicle very thick and fleshy. Violent peritonitis. Intestines during the operation very troublesome. *Recovery.* Died three years afterwards of phthisis pulmonalis, an hereditary disease. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. ix. p. 309.)

CASE III.—Operation, 15th of March, 1849; patient, Mrs. E. K., aged 29 years. Incision from symphisis pubis to middle of crest of ilium on right side, seventeen inches long; tumour ovarian, fibrous, firmly and extensively adherent to the bones of the pelvis and to the iliac vessels of the right side, weighing eight pounds. Poupart's ligament was embedded in the tumour and stretched across it. Complete procidentia uteri. A mixture of one part of chloroform and two parts of ether, liquid measure, inhaled with the happiest effects. *Recovery.* Still living.

Note.—As a matter of physiological interest, it may be well to state that

this patient has been twice pregnant since the operation, but in consequence of the great dread of parturition, abortion, without my knowledge, was artificially induced. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. xviii. p. 336.)

Case IV.—Operation, 22d of May, 1849; patient, Miss M. T., aged 33 years. Incision four inches above umbilicus to pubis; tumour uterine, non-adherent, not removed. Anæsthesia. *Recovery.* Died six months after from erysipelas. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. xix. p. 318.)

Case V.—Operation, 16th of June, 1849; patient, Miss H. M., aged 25 years. Incision from two inches above umbilicus to pubis. One large cyst extensively adherent to the omentum by its anterior surface; four gallons of chocolate-coloured fluid removed one week before; weight of tumour, forty pounds. Anæsthesia. *Recovery.* Still living.

Note.—Before the operation this patient had been the mother of an illegitimate child. She has been married since, and has given birth to two large, healthy children, a boy and a girl, passing through the period of both gestations without inconvenience, and having easy and speedy labours. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. xix. p. 328.)

Case VI.—Operation, 13th of October, 1849; patient, Miss M. B., aged 43 years. Incision from one inch above the umbilicus to pubis; tumour uterine, with large cystiform bodies incorporated with it; non-adherent, not removed. Anæsthesia. *Recovery.* Died between three and four years afterwards from the progress of her disease. Weight of the mass, removed after death, fifty pounds.

Case VII.—Operation, 24th of November, 1849; patient, Mrs. T. H., aged 39 years. Incision the same length as in Case VI.; tumour extra-uterine, fibrous, non-adherent, weighing six pounds. Pedicle very thick and fleshy, attached to fundus uteri. Menses present at the time of operation, and continued without interruption. Anæsthesia. *Recovery.*

Note.—This patient had recovered perfectly, was about the house, ate a hearty dinner of duck, filling, &c., which brought on an attack of cholera morbus, resulting in death thirty-nine days after the operation. In her best health before the operation, she for years avoided eating duck, because it invariably caused cholera morbus.

Case VIII.—Operation, 6th of February, 1850; patient, Miss L. N., aged 30 years. Incision same length; tumour cystiform, very adherent; two round, strong, vascular cords bound the cyst to the recto-vaginal *cul-de-sac*, requiring ligatures. Weight, fourteen pounds. Anæsthesia. *Death* from peritonitis on the sixth day.

Note.—This was not an unfavourable case for the success of the operation; and up to the evening of the fourth day, there was nothing to interfere with recovery. That evening she imprudently ate an orange, including the pulp, and soon after inflammation commenced.

Case IX.—Operation, 16th of February, 1850; patient, Mrs. F. C., aged 48 years, and greatly prostrated by her disease—indeed, danger of death

seemed impending. The stomach rejected all food, and the bowels were obstinately obstructed, as if by the pressure of the tumour. Incision same length; tumour cystiform, inseparably adherent to the viscera; adhering portions of the cyst were detached from the rest, and permitted to remain; spermatic artery cut and tied; weight, twenty-eight pounds. Anæsthesia. *Death* from exhaustion on the third day.

Case X.—Operation, 19th of March, 1850; patient, Mrs. S. L, aged 40 years, and so enfeebled by disease that her death was looked for daily. The ovarian dropsy was complicated with ascites; for the latter she had been tapped five times. The lower limbs were anasarcous and ulcerated, weeping away immense quantities of water. Incision same length; tumour cystiform; the omentum was much thickened, œdematous, spread over the cyst, and almost inseparably adherent; the uterus was also extensively adherent. Weight of tumour, twenty-five pounds, in addition to several gallons of peritoneal fluid. Anæsthesia. *Death* from exhaustion on the third day.

Note.—In both these cases the operation was performed only with the hope of arresting the progress of approaching death.

Case XI.—Operation, 13th of April, 1850; patient, D. S., aged 41 years, coloured single woman. Incision from near the umbilicus to pubis. Tumour uterine, non-adherent, not removed. Anæsthesia. *Recovery.* Still living.

Note.—In this case the intestines were very troublesome. They were forced out, and could scarcely be returned, in consequence of the anæsthetic agent inducing a cataleptic condition of the muscles, which extended to the muscular parietes of the abdomen. A favourable recovery, and improved health since the operation.

Case XII.—Operation, 15th June, 1850; patient, Mrs. D. H., aged 37 years. Incision same length; tumour cystiform, extensively adherent; a portion of the cyst, as large as the palm of the hand, could not be separated, and was left *in situ*. First cut the pedicle, then picked out the vessels and tied them separately. Weight of tumour, twenty-five pounds. Anæsthesia. *Recovery.* Still living.

Note.—This patient has been pregnant since the operation, but through over-exertion miscarried.

Case XIII.—Operation, 25th of July, 1850; patient, Mrs. M. B., aged 42 years. Greatly prostrated and anæmic from flooding and miscarriage two or three months before, and quotidian fever ever since; relieves suffering by the constant use of large doses of morphia. Incision same length; tumour cystiform, strongly adherent to the small intestines, uterus, bladder, rectum, and whole basin of the pelvis; a part of the cyst was left attached to the colon; several cysts broke while removing them and flooded the intestines, some of them filled with purulent matter; pedicle slender, applied no ligature to it, using torsion; pulse, at the time of the operation, 130, and exceedingly feeble. Weight of tumour, fifteen pounds. Anæsthesia. *Recovery.* Still living.

Note.—In this case the operation was performed only with the hope of arresting the progress of approaching death. This lady has since become a widow, and again married, and has enjoyed the most perfect health.

Case XIV.—Operation, 13th of November, 1850; patient, Mrs. J. S., aged 28 years. Had been tapped sixteen times; fifty-six pounds of fluid removed at one tapping; the mass of the tumour was composed of two immense cysts, and weighed eighty-one pounds. She was two months gone in pregnancy at the time of the operation. Incision from a point midway between sternum and umbilicus to pubis; extensive adhesions. Anæsthesia. *Recovery.*

Note.—This patient recovered from the operation, but this was followed by such great irritability of stomach, in consequence of a state of pregnancy, that she could not be nourished, and she died, thirty days after, of starvation. She had previously been reduced to a state of perfect emaciation. There was no miscarriage. The operation was performed in Connecticut. Her physician, Dr. J. Burwell, writes: "Hers was death by anæmia, complete exhaustion from want of nourishment." *Query.*—Would not the production of abortion have been likely to have preserved this patient's life?

Case XV.—Operation, 16th of April, 1851; patient, Mrs. M. W., aged 29 years. Incision from near the sternum to pubis. Tumour cystiform, firmly and extensively adherent; some peritoneal effusion; pedicle about six inches broad, filled with varicose veins; one very large cyst was filled with partially-washed crassamentum, and could not be diminished in size by the trocar. Weight of tumour, thirty-five and a half pounds. Anæsthesia. *Death* from peritonitis on the third day.

Note.—This operation was performed in the north-west part of Connecticut, and in the midst of that terrific storm that blew down the Boston Light-house. The room occupied by the patient was the best in the house, and yet the wind and rain had free access to it.

Case XVI.—Operation, 20th of May, 1851; patient, Mrs. T. G. A., aged 45 years. Incision from two inches above the umbilicus to pubis. Tumour extra-uterine, fibrous, non-adherent, weighing six pounds. Pedicle very thick, short, fleshy, and vascular, and attached to the fundus uteri. The omentum and small intestines forced out, remained so during the operation, and were returned with difficulty. Anæsthesia. *Death* from hemorrhage on the third day.

Note.—The anæsthetic agent produced the same effects upon this patient as stated in Case XI.

Case XVII.—Operation, 20th of December, 1851; patient, Mrs. A. M., aged 42 years. Incision from one inch above the umbilicus to pubis. Tumour extra-uterine, fibrous, firmly adherent everywhere, not removed. During the operation an abscess, deep in the abdomen, was opened, and discharged a quantity of pus. Anæsthesia. *Recovery.* Still living.

Note.—In this case, although the tumour was not removed, the operation was considered to have warded off impending death.

Case XVIII.—Operation, 3d of January, 1852; patient, Mrs. M. Q., aged 68 years. Incision about seven inches in length. Tumour cystiform, adherent, and weighed twenty-eight pounds. Pedicle very vascular. Anæsthesia. *Recovery.* Still living.

Note.—In this case, in consequence of the great age of the patient, the intense suffering, and rapid prostration, the operation was performed, at the solicitation of the patient, only with the hope of arresting impending death.

Case XIX.—Operation, 31st of May, 1852; patient, Miss H. S., aged 20 years, exceedingly feeble; constitution broken up, mouth and throat aphthous, appetite wholly destroyed, rapid emaciation, and very frequent small pulse. Incision from one inch below the umbilicus to pubis. Tumour cystiform, firmly and extensively adherent; cysts quite rotten, and filled with purulent fluid; omentum much thickened. Weight of tumour, twenty-five pounds. Anæsthesia. *Death* from exhaustion in thirteen hours.

Note.—With the hope that there was a remote chance of arresting death, I consented, at the earnest solicitation of the patient, to operate, although she was evidently sinking rapidly. The operation afforded entire relief to all pain, and added much to her comfort.

Case XX.—Operation, 16th of August, 1852; patient, Mrs. E. A., aged 30 years, greatly prostrated and emaciated; stomach rejects all food and medicine; is kept constantly under the influence of morphia; pulse very feeble. Incision from one inch above the umbilicus to pubis. *Both* ovaries removed; right ovary multilocular and medullary; left ovary unilocular, rotten and gangrenous, containing very offensive putrid gas; everywhere adherent. Weight of tumour, about forty pounds. Anæsthesia. *Death* from exhaustion in nine hours.

Note.—In this patient the disease commenced about the middle of March of the same year. The patient was in a similar condition to Case XIX, and was operated upon with the same hope.

Case XXI.—Operation, 3d of March, 1853; patient, Mrs. M. E., aged 40 years. Incision from two inches above the umbilicus to pubis. Three fibrous tumours, extra-uterine, adherent, weighing four pounds. Two of the tumours had very thick, short pedicles; the other was embedded in the substance of the uterus, and was enucleated. The small intestines were forced out, and with difficulty were replaced. Anæsthesia. *Death* from peritonitis on the third day.

Case XXII.—Operation, 14th of September, 1853; patient, Mrs. S. R., aged 56 years, enormously enlarged, and a great sufferer. Incision from umbilicus to pubis. Tumour cystiform, extensively adherent; considerable peritoneal effusion; pedicle broad and vascular. Weight of tumour, fifty pounds. Anæsthesia. *Recovery.* Still living.

Note.—The rapid increase in size, the great oppression, weight, and suffering, and the age, in this case, led us to apprehend a rapidly fatal termination, unless arrested by an operation.

Case XXIII.—Operation, 21st of September, 1853; patient, Mrs. E. S., aged 26 years, very much prostrated, and suffering great agony. Incision from umbilicus to pubis. Tumour cystiform, strongly and extensively adherent; several cysts gangrenous, rotten, and filled with pus. Weight of tumour, over forty pounds. Anæsthesia. *Death*, in twenty-two days, from gangrenous perforation of the jejunum. The opening was about one inch in diameter.

Note.—The disease, in this case, commenced about six months before, and progressed with great rapidity. The vital powers were sinking fast, and the operation was undertaken with the hope of warding off impending death. At the time, gangrenous inflammation was found occupying the right side of the abdomen, yet notwithstanding for two weeks after the operation, there were strong evidences of recovery. Indeed, the wound had healed, the ligatures had come away, and the patient was sitting up. The appearances of the perforated bowel indicated that its vitality had been destroyed at the time of the operation, and that nature had attempted to repair the mischief. It was considered by all present that life had been prolonged by the operation.

Case XXIV.—Operation, 17th of April, 1854; patient, Mrs. J. C., aged 36 years. Incision five or six inches long. *Both* ovaries removed; tumours cystiform, non-adherent, weighing fifteen pounds. Some peritoneal effusion. Both pedicles broad and vascular. Anæsthesia. *Recovery.* Still living.

Case XXV.—Operation, 13th of July, 1854; patient, Miss S. M., aged 31 years. Incision from umbilicus to pubis. *Both* ovaries removed; tumours cystiform, very strong, and extensive adhesions, extending to the whole interior of the pelvis. Weight of the tumour, fifty pounds. Anæsthesia. *Death* on the fifth day, of exhaustion and disease of the stomach and ileum.

Note.—The *post-mortem* examination revealed extensive disease in this patient. The whole cavity of the abdomen was sprinkled over with tubercular deposits. A large mass of it was attached to the fundus of the bladder. Another mass penetrated the coats of the ileum about thirty-six inches above the valve of Bauhin, and from this point down, the bowel was greatly contracted; above this point the bowels were distended with a yellowish liquid. The stomach and duodenum contained over half a gallon of black liquid, resembling the black vomit of yellow fever. The mucous membrane of the stomach, to half its extent, was discoloured by this black matter, which seemed to penetrate the tissue. It was softened and easily abraded, and the other coats of the stomach were likewise softened wherever this inky discoloration existed.

Case XXVI.—Operation, 5th of September, 1854; patient, Mrs. W., aged 52 years. Incision four or five inches long. Tumour cystiform, slightly adherent, weighing twenty-four pounds. Anæsthesia. *Recovery.* Still living.

Case XXVII.—Operation, 30th of September, 1854; patient, Mrs. J. P., aged 59 years. Incision about six inches long. *Both* ovaries, and an independent pelvic tumour removed. Right ovary fibrous, of bone-like hardness, and short, thick, fibrous pedicle. Left ovary cystiform and fibrous, having a broad, vascular, flesh-like pedicle. Pelvic tumour fibrous, as hard as bone. Very firm and extensive adhesions. Weight of tumour, twenty pounds. Anæsthesia. *Death* on the fifth day, from secondary hemorrhage.

Case XXVIII.—Operation, 19th of October, 1854; patient, Mrs. S. M., aged 24 years. Incision from above the umbilicus to pubis. Tumour cystiform, adherent, weighing thirty pounds. Anæsthesia. *Recovery.* Still living.

Case XXIX.—Operation, 31st of October, 1854; patient, Mrs. A. E. L., aged 42 years. Incision from six to eight inches in length. Tumour cysti-

form; firm and extensive adhesions. Weight of tumour, thirty-eight pounds. Anæsthesia. *Death* on the sixth day, from secondary hemorrhage.

Case XXX.—Operation, 16th of December, 1854; patient, Miss D. P., aged 49 years. Incision about five inches long. Tumour cystiform, non-adherent, weighing eighteen pounds. Small intestines forced out and troublesome. Anæsthesia. *Recovery.* Still living.

Note.—The anæsthetic agent produced the same effects upon the patient as stated in Case XI.

Art. VIII.—*A Case of Removal of the entire body of the Uterus by the large Abdominal Section.* By E. R. Peaslee, A. M., M. D., Prof. of Surgery in the Med. School of Maine, &c. (With a wood-cut.)

The surgeon who assumes the responsibility of a novel or rare, and, at the same time, dangerous operation, but has not the courage to report the result, unless it be successful, fails to improve an opportunity for contributing to the advancement of his profession. Every such operation is, scientifically, an *experiment;* and to give the results, in any department of science, of those experiments only, out of the whole series of the same kind, which resulted as the experimenter previously hoped they might, would be always wrong, often injurious, and sometimes productive of even fatal consequences.

It is only by knowing *all* the results in the same series of experiments that we can arrive at correct conclusions. It is, therefore, the duty of surgeons to record all their important operations in a form for future reference, if required; and especially to publish the results of all such as are new or very seldom performed. Thus alone can the true value of any operation not yet generally recognized, be decided; and so long as it can be said of any operation that but few of all the unfavourable cases have been reported, a prejudice against it (though perhaps unjust in degree) will very justly be entertained.

To secure the object in view, then, unfavourable cases are indispensable. They are also just as valuable as the favourable ones; provided they are reported with the same care and discrimination—which is, however, very rarely the fact. And we will hope that the time will come when the desire to advance the science and art of surgery may become paramount to that of blazoning forth the operator's boldness or skill, and may more frequently become the real motive for reporting operations in the public journals.

The following is a case of removal of the whole of the body of the womb, by the large abdominal section; the patient having died on the fifth day after the operation, from gangrene produced by the strangulation of two hernial protrusions.

Mrs. Dean, of B———, Vt., a widow, aged 35, the mother of four children, of good conformation, and bilious temperament, first applied to Dr. J. F. Skinner, of B———, in April, 1853; and I obtain from Dr. S.'s first letter to me respecting her, the following history of her case from the commencement till I saw her in August following.

"I found her," says Dr. S. (in April), "with a tumour in the right iliac region, which I judged to be ovarian. She was also suffering severely from tenderness of the whole spinal column, and much pain in the head; the bowels were excessively costive; there was leucorrhœa, with difficulty in micturition, and numbness in the right limb. I learned that her last labour (17 months ago) had been a tedious one, and was followed by much inflammation of the pelvic organs; and that she was several weeks in recovering so far as to be able to walk. The *tumour* was first noticed soon after her confinement, and since my observation, has rapidly increased."

All the troublesome symptoms were relieved by Dr. Skinner, except the difficulty in micturition; "the tumour being so movable that on assuming the erect position, it falls partly into the pelvic cavity, often stopping the flow of urine, and producing such violent spasmodic pains, that she has been obliged to have the bladder evacuated by the catheter exclusively for several weeks past. The same suffering also attends every evacuation of the bowels; and, of late, even the erect position brings it on. When the bladder is distended, the tumour will rise above the umbilicus; and on evacuating the former, it will fall as low as the pubes; and I judge from its mobility that it cannot be extensively adherent, and that an operation for its removal might be advisable. Indeed, I can see no other chance of continuing life long, or even making the patient comfortable while life lasts. Dr. Newell, of Lyndon, has also examined the case, and accords with me in opinion. I would add, she has menstruated several times since her confinement, but not very regularly as to time, and with considerable pain."

I replied that ovarian tumours do not generally produce such suffering from a change of position, and that I had never seen a case attended by all the symptoms he had mentioned; that though I had successfully performed the operation of ovariotomy,[1] I considered it very rarely justifiable, though sometimes *clearly* so, and should never perform it from any other motive than a sense of duty.

Dr. Skinner replied that "he had doubts in his own mind as to an operation, and wished for advice;" and it was arranged that I should visit his patient on the 2d of August: Dr. J. P. Bancroft, of St. Johnsburg, Vt., my friend and former pupil, accompanied me, and assisted in the following examination:—

Examination of Patient, August 2d, 1853.—She is confined to her bed; is pale, and rather emaciated; tongue coated; has little appetite; can sit up but little, so much distress is occasioned by pressure from the tumour when the trunk is erect.

The tumour now lies on the middle line, is about four inches in its bilateral dimensions, and extends upwards to within one inch of the umbilicus. It is very firm, is round and smooth, except at a point two inches below the upper extremity, and to the right, where a small hemispherical protrusion apparently about one inch in diameter can be distinctly felt through the abdominal walls. The tumour is perfectly movable in all directions, but naturally inclines more to the right; and is not sensitive under pressure. The bladder

[1] See a Report of this case in this Journal—April, 1851.

is now distended with urine; but after its evacuation by the catheter, the tumour descends so as hardly to appear above the pelvis; and if the patient now rises, much distress is produced by its downward pressure.

After a thorough evacuation of the loaded rectum by an enema, I proceeded to make an internal examination.

1. *Per vaginam* the finger discovered the os uteri at the normal distance, and in its usual relations (it had been somewhat elevated when the tumour was pushed up by the distended bladder). It was patulous, and had a velvety feeling. The tumour could be distinctly felt in the region of the right ovary, it inclining to that side as it entered the pelvis; and could not be felt at all on the left side. It appeared solid as before, and was not tender on pressure.

2. An examination *per rectum* also enabled the finger to reach and to elevate the tumour on the right side of the uterus, and the body of the last seemed carried somewhat to the left. The same solid feeling before mentioned was also here detected. The tumour was very movable, as before.

3. On introducing a *speculum vaginæ*, the os uteri was seen to be slightly ulcerated, and to be in other respects normal. The *uterine sound* passed through the cervix with much difficulty on account of the very small diameter of the passage; but on arriving in the uterine cavity passed at once to the distance of three and a half inches from the os, and then appeared to reach the fundus, as it could be passed no further. The uterus is very movable, and can be carried in any direction by the sound, except to the left, while the tumour is held still in its place in the pelvis; or, the uterus being fixed by the sound, the tumour may be moved freely to and fro, without moving the womb; unless when carried against the latter to the left or in front.

4. A sound being introduced into the *bladder*, the tumour is also movable in all directions, without imparting its motion at all to that organ.

Diagnosis.—In view of the history of the case, and the facts elicited by the examination, I judged this was a case of tumour commencing in the right ovary, and not yet adherent to any other part or organ, except to the uterus, by its natural but enlarged attachments.

In this opinion I was confirmed by those of Drs. Skinner and Bancroft, who assisted me, and with whom every fact and its bearing was freely discussed during the protracted examination made. Other affections, for which ovarian tumours may be mistaken—especially fibrous and other tumours of the uterus—were constantly kept in mind, and the decision above mentioned was arrived at as the only one consistent with all the facts in the present state of our knowledge of intra-pelvic tumours.

The patient insisted on the removal of the tumour, and I was obliged to admit that this appeared a case as favourable for ovariotomy as is often seen, so far as the size and mobility of the tumour were concerned. But I should not, at any rate, operate till she was in a better general condition; and indeed would not, without further reflection, encourage the idea of operating at all, for the reasons I had previously stated in my letter to Dr. Skinner.

The patient had already been made aware of the risks of the operation by her attending physician; and to the remark that it might itself prove fatal, replied that she was already the same as dead to her two surviving children as far as assisting them was concerned; that she lived for them alone, and therefore wished to seize on any chance for a return of health and strength, however slight it might be, and however great the accompanying risk.

In view of all the circumstances, I finally consented to undertake the operation at some future time, provided she were first in a state of improved

general health so far as might be desirable, and that all the other circumstances became favourable; especially, also, that she should be removed to some not too distant point on a railroad, that I might visit her daily for any required time after the operation.

Everything subsequently becoming favourable, and every arrangement having been completed, the operation was arranged for September 21, precisely three years after the successful operation of ovariotomy before alluded to; it being understood with the patient, however, that if she then had a cough, or was in any way unwell, I should defer it till another time.

Plan of the Operation.—I proposed to make an incision four inches long through the skin and superficial fascia only, and then an opening in this, one to two inches long, into the peritoneal cavity. Then, if I discovered any error in the diagnosis, or that the tumour, for any cause, could not be removed, I should have but a small wound to close. On the other hand, if the tumour were a single sac of fluid, I would evacuate it by a trocar, and draw the sac through this small opening, and tie the pedicle; while if solid, I would enlarge the opening to the smallest required extent only, and thus remove it.

Sept. 19. On making another examination of the patient to-day at St. Johnsburg, Vt., where the operation is to be performed, in order to detect, if possible, any fallacy in the former conclusion as to the precise nature of the case, all things were found as at the first examination in August; except that the patient now appeared in good general health, though as much distressed as ever after micturition by the downward pressure of the tumour, and therefore still confined constantly to her bed. The tumour had, however, evidently increased somewhat in size, and now gave an evident, though not very distinct feeling of fluctuation. The latter fact only served to confirm me in my previous diagnosis.

She was to take a dose of castor-oil on the evening of the 19th, and only milk porridge for diet till the operation (on the 21st.)

At the time appointed the patient was placed upon a table before a window, the pubes having been shaven, and the pudenda having been covered by a cloth folded like a diaper. The precise position of the linea alba was indicated by a line on the skin made by the tincture of iodine, with some lines across it to aid in a more accurate introduction of the needles through the abdominal walls; and the patient commenced the inhalation of the pure sulphuric ether through a tube made by rolling up a common towel and confining it with pins, at half-past three o'clock in the afternoon.

Drs. Jewett and Bancroft, of St. Johnsburg, Drs. Skinner, Pierce, and Masta, of Barton, Drs. Newell and Cahoon of Lyndon, Dr. Carpenter, of Sutton, and Drs. J. V. Lansing, and W. H. Burleigh, my private pupils, assisted in the operation. The temperature of the room during it was kept at 80° (Fah.), and the atmosphere rendered humid by the evaporation of water. I had also prepared an *artificial serum*[1] with which to moisten my hands when it became necessary to touch the peritoneum, or to apply to the latter while exposed to the air.

Before commencing the operation, I remarked to the medical gentlemen present I did not think it possible to arrive at any other rational conclusion than that this was a case of disease of the right ovary; but since the best diagnosticians had erred in cases of pelvic tumours, I should commence the operation fully prepared to modify my previous plan, or even at once to close

[1] A mixture of albumen ʒv and common salt ʒiij, with water Oiv.

up the wound if a different state of things from what I expected should be found to require it.

I commenced with an incision four inches long through the skin and superficial fascia extending from a point one inch below the umbilicus to another two inches above the symphysis pubis; these layers being pinched up, and together divided from within outwards by a long and slender knife. On doing this, the sheath of one of the recti muscles was opened for the extent of one quarter of an inch; and finding it to be the *right* rectus by the introduction of a probe into the sheath, I thus ascertained the precise position of the middle line. After waiting several minutes for the oozing of blood to cease, I divided the abdominal aponeurosis to the extent of one inch and a half, and found a layer of fat one-quarter of an inch thick presenting itself, and apparently *not* attached to the inner surface of the aponeurosis. Was this, therefore, the subperitoneal layer of areolar tissue unusually laden with fat, or had I already divided the peritoneum together with the aponeurosis?—in which case this layer of fat must be the great omentum. A probe could be freely passed in all directions between the aponeurosis and the fat. Still, on everting the aponeurosis, there was found to be no layer of peritoneum on its internal surface, and therefore the peritoneal cavity had not yet been opened. I therefore divided the layer of fat, and the peritoneum under it, to the extent of one inch and a half, and the tumour was brought into view. Some delay was necessitated by the layer of fat, it being different in appearance from anything I had met with before; but of course, the peritoneal *cavity* had not, till this instant, been exposed.

The tumour was smooth, except where the protrusion was situated which had been felt through the abdominal parietes, and which was seen to be a hemispherical sac filled with fluid, and about three quarters of an inch in diameter. It was of a pale livid colour, and perfectly free and unattached above, as had been predicted. To the touch, a feeling of fluctuation was very distinct; and all the medical gentlemen present accorded with my own opinion, that it was a very dense sac containing fluid.

I accordingly thrust a trocar into the tumour to withdraw the fluid, but *not a drop appeared*, except a few drops of blood as the canula was partially withdrawn. Thus, finding the tumour to be solid, I next enlarged the opening through the abdominal aponeurosis and peritoneum, in order to examine its attachments below. Perhaps a drachm of blood escaped into the cavity of the peritoneum while effecting this, and two minute arteries were tied to prevent hemorrhage in this direction.

The tumour was now drawn out through the incision, when, to my disappointment a second time, I found that it was continuous with the *fundus of the uterus*. Indeed, at first sight, it appeared to be merely *the uterus* very much enlarged; but, on examination, it was found to be a development *from*, and not *of*, this organ.

What was next to be done? To return the mass and close up the incision was the expedient which alone would seem to remain; but the puncture made by the trocar was now bleeding freely, and the idea of returning the tumour in that condition was not to be entertained; while, to arrest the hemorrhage for the time, by sutures, or other mechanical means, would have been merely postponing the risk without removing it. The body of the uterus was elongated and slender at its lowest part in the pelvis, from the traction upwards, probably of the tumour by the distended bladder; and it was unanimously decided, by all the medical gentlemen present, that the risk to the

patient would be no greater of removing the whole mass, including the body of the uterus, at its lowest part, than of returning the former in the circumstances just mentioned, and I decided to remove it. The patient just now began to retch quite violently, and the intestines to protrude. I accordingly at once extended the incision to the symphysis (to six inches), and passed a stout double ligature, of four threads of saddler's silk, waxed, but not twisted, through the uterus as low in the pelvis as possible, and amputated the organ above the ligatures. The left ovary being diseased, was removed with the uterus; the right was left *in sitû*, ligatures having been applied so as to prevent any hemorrhage when the necessary incisions were made through the broad ligaments. The intestines were returned by the hands, after being moistened in the artificial serum, which was kept blood-warm and constantly used after the cavity of the peritoneum had been opened. An artery in the remaining portion, the *stump* of the uterus, had not been sufficiently constricted by the ligatures at first applied, and bled quite freely till a separate ligature was applied to it.

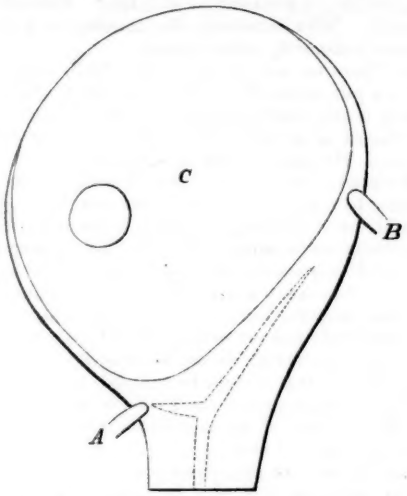
The clots were removed from the pelvis, the hemorrhage not exceeding six ounces in all; and the wound, six inches long through the superficial parts, and five inches through the peritoneum, was closed by six needles placed three-quarters of an inch apart, and seven sutures; five being between the needles, and the remaining two being at the top and at the bottom of the incision. The ligatures came out at the nearest point, between the lowest needle and the lowest suture; and a short gum-elastic tube was left introduced by the side of the ligatures. Adhesive strips were applied over the needles and sutures; a compress, saturated with warm water, and covered with oil-silk, was laid over these, and the patient transferred to her bed.

Examination of the Tumour.—The weight of the whole mass removed is nineteen ounces avoirdupois; it being five and a half inches long, four inches wide, and three inches thick. With the exception of the hemispherical sac, before alluded to, three-fourths of an inch in diameter, it is externally of uniform appearance, and still gives an indistinct feeling of fluctuation. In size and shape it much resembles the uterus enlarged by impregnation. The lower portion being slender, all the part above can be easily *flexed upon that in every possible direction.* The two halves of the mass are, however, not symmetrical, like the impregnated uterus; the right side being the most prominent, and the sense of touch easily distinguishes a large tumour forming in the whole upper part and the right half of the mass. A sound, also, introduced into the portion of the uterine cavity left in the mass, shows that it extends to the left side of the mass and to the distance of three inches; there being a sudden turn to the left at the distance of one inch. The Fallopian tubes were of normal size; but, while the right was nearly in its normal position, so far as distance from the cervix uteri was concerned, the left originated from the mass three inches higher up.

On making an incision through the part where the distinct tumour could be felt, the latter was found to be a fibrous growth, already softening and degenerating in its central portions. It must have originated at the right side of the fundus of the uterus and in its substance, though near the surface; and, as it increased in size, elongated the uterus, also elongating its cavity and turning it to the left. The sinuses of the uterus in its vicinity were much enlarged, and hence the fluctuation already spoken of.

The accompanying diagram gives an idea of the mass removed. A and B represent the right and left Fallopian tubes; C, the tumour bounded below

by the fine line; and the dotted lines show the form of the uterine cavity. The small sac, containing fluid, is seen at the right and below C.



Condition and Progress of the Patient subsequently to the Operation.—The following account is condensed from an hourly record made by my pupils, Drs. Lansing, Burleigh, and Lunt, who constantly attended the patient in turn till her death.

To avoid repetition, I here remark that she was to take only milk porridge, or bread-water, and, later, broth, as nutriment; to take McMunn's elixir of opium, or acetas morphiæ, to control pain or restlessness after reaction was established; and brandy, to aid in inducing the reaction, if required. The bladder was to be evacuated once in six hours, or oftener, if desired, by *catheterism*, for the first five or six days; and the warm water dressing was to be renewed every four hours. I shall call attention particularly to the pulse, the respiration, and the amount of urine, and divide each day into four parts, commencing at 6 A. M.

The patient completely recovered her consciousness soon after being put in bed, at 5½ o'clock P. M., and complained only of fatigue and pain in the back. At 9 P. M. reaction is established, and she is comfortable. Pulse, 96; respiration, 20; evacuated ℥iij urine. Gave gtt. xxv McMunn's Elixir. At 10, pulse 104. She has a desire to evacuate the bowels. Gave an enema of ¼ gr. acetas morphiæ in water, ℥j. Drew ℥j urine, she complaining that all had not flowed before. At 11, had slept fifteen minutes; surface warm; pulse, 102; respiration, 18. Gave acetas morph. gr. $\frac{1}{12}$. At midnight, she had slept one-half hour more. She is warm and comfortable, and sleeps one-half of the time from 12 to 1 o'clock. 2 A. M., says she is hungry; has slept most of the past hour. At 3, pulse 98; respiration, 18; obtained urine ℥iij. Having complained of thirst since the operation, she had drank, perhaps, ℥viii, in all, of water. She now vomited most of this, and felt better afterwards. But she has also been *coughing* frequently; hav-

ing taken a cold, two nights ago, by sleeping with her window open. As she knew I would have deferred the operation had I known this fact, she had concealed it from me. After the vomiting, till 6 A. M., she remained very comfortable, often sleeping a few minutes at a time. Pulse from 96 to 88; respiration always 18. After examining the dressings at 4 A. M., I left in an early train for my home, sixty miles distant.

First day after Operation.—6 A. M. to 12. At 6, drew urine ℥ij. Is now complaining of a pain which she refers to the womb. At 7½, the urine is passing involuntarily in very small quantity; continues only two or three hours. Up to 9, she complained of no pain; but of wind in the stomach which she made efforts to raise. The pulse is now 104, respiration 18. Has coughed frequently since 6 o'clock. At 10, the pulse is weaker. Gave ℥j brandy. There is a disposition to vomit, after taking some porridge; which is allayed by some ice. At 11, pulse 108, respiration 15; sickness and efforts to vomit continue. On examining the dressings at 12 (noon), Dr. Lansing found the tube had discharged ℥j of bloody serum, and injected through it an ounce of the artificial serum, which came away again. But he also found 2 *hernial protrusions*—a small one near the lower end of the incision, and a larger near the upper end. The former was reducible; but the latter was not. And, as efforts to reduce it made the patient vomit, he at once telegraphed to me the state of things. I replied that it must be reduced, and the air excluded by the dressing till I could arrive there; which must be as late as 4½ to 5 P. M. From 12 to 5 P. M. (the time of my arrival) she continued to have all the usual symptoms of strangulated hernia; the pulse becoming more rapid, but feeble, and the patient restless and oppressed with thirst. Retching still continued; and she had been supported by brandy. I found a portion of intestine as large as a hen's egg protruding between the uppermost needle and the upper extremity of the incision; and another of the size of a walnut, between the lowest two needles. Both were strangulated and irreducible herniæ; both were very livid, and covered with a plastic exudation. Pulse 128, and very feeble. I withdrew the needle below the protrusions in each instance, and returned the strangulated intestine; and then replaced them and added two other needles for greater security, and left a tent in place of the tube. All this still farther reduced the patient, and I supposed that this unfortunate occurrence must turn the scale with her, as a matter of course; and very likely she would not react at all after the shock. From 6 to 8, patient rested well, dozing much of the time. Two oz. of urine had been withdrawn just before the herniæ were reduced. At 9 in the evening, pulse 130, fuller, respiration 17, tongue moist, slight white coat. At 10, the pulse was 125, surface natural. Is lying on the right side, and complains somewhat from uneasiness of position. Changed to lie upon the back, she soon wished to resume the former position. At 11, temperature was diminished; gave brandy and water. Is troubled by her cough. Gave ¼ grain of morphia. Slept at short intervals. 12 o'clock (midnight), catheterism, f℥iv. Pulse 125, more feeble; respiration 16. Gave nutriment and brandy. Add more bed-clothing. 1 A. M., pulse 136. Coughed several times, and raised freely a tenacious mucus. Gave ⅛ grain morphia. Slept well afterwards; temperature rising. From this time till 4, she made frequent ineffectual attempts to expectorate mucus, which somewhat exhausted her; the temperature of surface continued sufficiently high; slept ¾ hour quietly at one time. Pulse 125 to 130. Took brandy twice, and ⅛ gr. morphia at half past 3. At 4 A. M., pulse 140, respiration 17; is placed upon the side; complains of being tired; is very thirsty. She continues thus,

with temperature rising till 7, when she had been sleeping quietly for half hour; pulse 135.

Second day after Operation.—6 A. M. to 12. At 8 A. M. feels very comfortable. Compress on abdomen stained with bloody serum that had passed by the tent. Abdomen somewhat distended by gas; no increased tenderness. Introduced catheter; no urine obtained. Expectorates much thick mucus. Slight eruptions on the chest, produced by the morphia. Pain on coughing, all referred to the umbilical region; no pain elsewhere. At 9½ to 10, sleeping quietly; skin moist and natural; pulse 128, respiration 16. At 10¾, obtained ℥iv of urine. She was soon after turned on the side, and both brandy and nourishment were given. At *noon* the pulse was 136. Awoke quite refreshed after an hour's sleep, and took some chicken broth. The tongue is more coated. She continued sleepy, and with a moist skin till 3 o'clock, when about ℥vi urine of natural appearance were drawn. Viscid mucus was freely raised several times. At 5 (48 hours after the operation), pulse 140, and fuller, respiration 21; surface less warm. At 6¼, ℥iv urine were drawn, and patient changed to other side of bed. The gaseous distension of the abdomen not increased; the parts look well. Up to midnight, she had not slept much, and complained of pain in the abdomen. Took morphia twice, and brandy and broth freely. Pulse generally small and feeble, over 130. At 11, drew ℥v of urine. Has difficulty in raising mucus still. 12 (midnight), abdomen still tympanitic; in other respects appeared right. She dozes somewhat; complains of feeling faint. Pulse slightly irregular, 118 to 98, respiration 20 to 17; dreamed of home; gave brandy and broth freely. At 5, respiration was 12; probably reduced by the morphia; pulse 125, feeble.

Third day after Operation.—6 A. M. to 12. Temperature natural; tenderness and swelling of abdomen not increased. Withdrew f℥iij of urine. Patient inquires how long before she will be able to go home. Tongue as yesterday, except drier. She did not complain at all this forenoon, except in connection with her efforts at expectoration. The pulse improved in character, and continued from 118 to 128, the respiration being 12 to 14. Morphia ⅛ grain was given at 9 o'clock. At 10, she passed urine quite freely; and slept quietly from 11 to 12. Has, in fact, had a very comfortable forenoon. 12 to 6 P. M. At 12 (noon), she passed more urine, and the catheter withdrew ℥ij more. Swelling of abdomen not increased. In the P. M., she makes slight attempts at times to cough, but has difficulty in expectorating. At 3, drew ℥v urine. Took morphia at 1 and 4½. The pulse is more distinct and regular, and from 130 to 120; respiration 16. Has slept some twice, and complains of itching (from the morphia). Starts in her sleep, but does not moan; countenance natural; and has had a very comfortable afternoon. 6 P. M. (72 hours after operation). Abdomen less distended than 24 hours ago, and not tender on slight pressure. From this time to midnight, patient slept most of the time very quietly, the skin being moist and natural. At 9, ℥iij urine were drawn. The pulse was 124 to 128, and the respiration 16. 12 o'clock (midnight). Remains the same. At 2 A. M., and at 5, about 1 oz. of urine; and for two hours (and then after 4 again) there was great heat and dryness of skin, and thirst. She kept two hours at intervals; being restless at 4, took morphia. Pulse from 128 to 130, and feeble; respiration 15 to 16. Broth and brandy were freely given. Once expectorated freely a thick yellow mucus; and expelled some gas from the stomach.

Fourth day after Operation.—6 A. M. to 12. I spent this day with the

patient. Early in the morning the pulse is irregular, and she first ejects from the stomach a yellow fluid (apparently bile), which she says is very acrid. This continued at intervals all day, the quantity at each time, varying from ʒj to ʒiv. Brandy and broth are freely given, but the pulse continues small and irregular, and from 125 to 130; respiration being 18. Gave ⅛ grain calomel every two hours. Urine ʒj at 9 A. M. Complains of griping pain in the bowels. Give aq. menthæ piperitæ. Applied a sinapism to the epigastrium. In the afternoon, used chloric ether, the vomiting of bile continuing; though she says she does feel nauseated. It became quite violent at 3, and the ejected matter of a darker colour. Pain increases in the abdomen; also felt in the chest. Applied fomentations to latter. Pulse from 130 to 140, and feeble; respiration 25 to 30, expiration longer than inspiration. Urine ʒiv at 4½ P. M. Is less pain in side. 6 P. M. (96 hours after operation). I noticed a faint gangrenous odour on examining the dressings. Gave an enema of soap and water at 9 P. M. She vomited at once. Gave ½ drop creasote every two hours afterwards during night. Vomited often up to midnight. Urine ʒiv at 10 P. M., reddish coloured. Just before 12, complains of deafness in the left ear; has slept some; extremities cold. Pulse has been 120 to 132, very feeble; respiration 25 to 32. 12 o'clock (midnight). Feet warm, hands still cold. A cold perspiration appears at 1; she vomits afterwards; wishes the window opened at 2; pulse scarcely perceptible at 3; respiration being 24; has moaned somewhat the last half hour, and tosses her head about in distress; thinks at 4 that she is dying, and expresses a desire to die, and a Christian hope. The vomiting continues at intervals. She is moribund at 5; vomits still; but lingers till 20 minutes past 7 A. M., when she expired on the fifth day after the operation.

Post-mortem Examination eleven hours after death.—Present, Drs. Jewett, Newell, Bancroft, and Cahoon, and my pupils, Drs. Lansing, Burleigh, and Lunt.—Slight œdema of skin upon the abdomen, and slight tympanitis. Made a narrow elliptical incision through abdominal walls, and include the original incision and two inches on each side of it. The right rectus muscle was in a gangrenous condition. The original incision was completely united, both externally and internally, except where the two herniæ had occurred; an inch remaining ununited at the upper end of the incision.

The *omentum* was adherent at its right lower border to the small intestine. A little blood, just enough to produce a stain, was extravasated upon the right portion of the stomach and the omentum.

Intestines.—The portion of intestine forming the largest (upper) hernia was livid and gangrenous, and slightly adherent to the edges of the upper part of the incision, which was not yet closed. There was a similar slight adhesion at the position of the lower hernia. There was some adhesion to the peritoneum at intermediate points. No strangulation at any point, but a general livid appearance of the small intestine, apparently having commenced from the two portions constituting the herniæ at first, and corresponding to the track of the wound. Some convolutions were also adherent to the left iliac fossa, and to each other; and others falling into the pelvis adhered slightly to and behind the portion of the uterus still left in the pelvis.

The small intestines were somewhat distended with gas.

Four ounces of bloody serum were found in the pelvis; and the peritoneum there was of a livid hue.

The stump was completely covered with a thick layer of plasma already beginning to be organized, and healthy in appearance. The length of the re-

maining portion of the uterus was two and a half inches. The stump was 1¼ inch wide, and 1 inch thick.

Remarks on the preceding Case.

1. Each reader will decide how far the mistake in the diagnosis was a culpable one. I availed myself, I believed, of all the means and appliances afforded in the present state of science to elucidate such cases, and the facts discovered were such, as in my opinion, could lead to no other rational conclusion than that we had here an ovarian tumour. The history of the case, the data afforded by the touch, whether applied externally, *per vaginam*, or per rectum, and by the uterine sound, and the sound in the bladder, all confirmed the idea of an ovarian tumour; and though some of the facts were also compatible with the supposition that the tumour was uterine, others seemed not to be so at all.

So far as form, position, and mobility were concerned, the tumour might be either ovarian or uterine. But the fact that the tumour could be moved in all directions without moving the uterus, while the sound was in the latter, the sound having passed three and a half inches into the uterine cavity and seeming to reach its upper terminus, was considered entirely inconsistent with the idea that the tumour was in the substance of the uterus.

But why did not the sound penetrate further into the uterus? Because, as the post-mortem examination showed, the cavity of the uterus turned to the left at the point reached by the extremity of the instrument. And the displacement of the mass in all directions, while the sound was in the uterus, without imparting motion to the uterus except in the slightest degree (as slight as would have been produced by an ovarian tumour, on being pressed against that organ), was subsequently accounted for by the elongated and slender form of the lower part of the body of the uterus, and its consequent flexibility at and above that point. In another similar case, I should *now* use a *flexible bougie* in exploring the length of the uterine cavity, after employing Simpson's uterine sound, as before; and finding the latter would pass only the usual distance. It will appear that the diagnosis actually turned upon this single point. If I had succeeded in passing the uterine sound to the upper extremity of the uterine cavity (five and a half inches), I should have consequently detected its enlargement, and also have fixed it so firmly that the flexibility of the lower part would not have deceived me.

But my idea of an ovarian tumour was also confirmed by the distinct *fluctuation* perceived at the examination two days before the operation. Of course a uterine tumour may also fluctuate, though very rarely when so firm and large. And this fluctuation still deceived me, even after the tumour was exposed to view during the operation. It was found to be owing to the fact that the uterine sinuses were enormously enlarged in the layer of uterine structure covering and surrounding the tumour, and were of course full of blood.

Still another thing influenced my opinion in no small degree. I allude to

the small sac containing fluid, which I could distinctly feel through the abdominal walls, on the anterior surface of the tumour. I had before repeatedly felt such projections on ovarian tumours, but never on a uterine tumour; and such a sac had always hitherto proved a true indication of the nature of the mass on which it was found, viz: that the latter consisted of a congeries of sacs of various sizes containing fluid.

I trust that by calling the attention of the profession to these causes of error on my own part, some may be put on their guard against similar results in diagnosis. In another precisely similar case, I should now adopt it as a principle to decide positively and conclusively by various explorations of the uterine cavity, that it was *not a uterine tumour* before I decided that it *was ovarian*. But if, after all, I could obtain only precisely the same data as before, I do not now perceive how I could rationally arrive at any other conclusion than the one erroneously adopted in the present case.

2. But should the uterine mass have been removed when the real state of things became apparent? I have no desire to avoid this inquiry. Many will doubtless, at once reply that the mass should have been returned, though already punctured and bleeding, and the wound through the parietes of the abdomen closed up. All the medical gentlemen present, however, thought differently. I mention this, not to throw any part of the responsibility upon others, but to show that I acted deliberately, at any rate. I should never forgive (any more than I should respect) myself, should I perform an operation, or any part of one, which I myself did not, in the circumstances, consider justifiable, whatever might be the opinions of others. And there were circumstances not likely to occur to the mind of one not present at the operation. The patient was retching violently, and it seemed utterly impossible to return the tumour and retain it there by needles, and the usual appliances in the present spasmodic condition of the muscles, even if the tumour had not been punctured. And it is still my opinion that, if it had been returned *sound*, the cough would have produced the hernial protrusions, which subsequently did occur, to a still greater extent and certainty. The abdominal walls had not been distended as in cases of ovarian dropsy; and, therefore, the removal of the tumour rendered hernial protrusions less liable to occur from leaving the abdomen so much the less full and tense. But to return the mass still bleeding was out of the question, and to stop the hemorrhage for the time, and return it with the certainty that it would again occur internally, was an expedient at once rejected.

I had, however, realized the possibility of an error in diagnosis; and in endeavouring to include all the *possibilities of the case* in my mind, the very thing I now found to exist had occurred to me, and I had previously decided what would probably be best in the circumstances.

But this decision involved the removal of the *entire body of the uterus through the abdominal walls*, an operation which had not been performed in this country, so far as I am aware. This organ had been successfully removed through the

vagina in cases of cancerous affection by Sauter, Recamier, and Blundell. The operation decided on had once been performed by Langenbeck. Dr. Clay, of Manchester, England,[1] had also performed it; the patient dying of hemorrhage seventeen hours afterwards. Aside from the risk of hemorrhage, the danger of life seemed to me even less great of removing the body of the uterus in this way than of removing the entire organ through the vagina.[2] The shock could be no greater,[3] and the peritoneal cavity would not be exposed at all after the incision in the abdomen was closed, as it must be to some extent after its removal through the vagina.

Such were the facts and the reasoning which had previously prepared my mind for the removal of the uterus, if the circumstances seemed to require it.[4]

3. But was not the removal of the uterus the cause of the fatal result? I should reply in the negative. If the removal of this organ proves fatal, I suppose it must be so from the *shock*, from *hemorrhage* at the time, or subsequently (as in Dr. Clay's case), or from *inflammation*, either peritoneal or extending from the seat of the amputation to the surrounding parts. But death from shock would have occurred in a few hours, at all events within two days. I provided for hemorrhage (the cause of death to Dr. Clay's patient), by the use of the elastic tube for the first twenty-four hours; but none occurred at all. I suspected at the time, however, that the desire to evacuate the bowels, soon after the operation, was produced by the pressure of blood flowing from the stump. Again, the stump was not inflamed at all, as found at the autopsy. On the contrary, all progress had been made which was possible towards healing over during the interval after the operation.

What, then, was the fatal element in this case? Merely opening the peritoneal cavity may prove fatal, but then the cause of death is *peritonitis*. In this case, the extent of the peritoneal inflammation was merely proportional to the slightly developed symptoms before death; and it alone, I judge, could not have proved fatal. Besides, its location was such as to suggest the idea that another cause than an exposure of the general cavity produced it. The symptoms, also, during the last two days, were those of gangrene, and not of peritonitis.

I am, therefore, constrained to regard the hernial protrusion, the morning

[1] Lond. Med. Gazette, vol. xxx. p. 309. 1844.

[2] Still there is a case on record in which the uterus was *torn* away by an ignorant midwife, supposing it to be another child; and yet the woman recovered.—*South's Chelius's Surgery*, vol. iii. p. 569.

Delpech thinks the removal through the abdominal walls the safer operation.—*Memorial des Hopitaux du Midi*, Oct. 1830, p. 695.

[3] Gendrin thinks that in the cases in which death ensued within forty-eight hours after the operation (through the vagina), it was consequent on the depression caused by the pain from dragging down the uterus during the operation.—*Journal Générale de Médecine*, Oct. 1829, p. 91.

[4] Since commencing this report, I have learned that Dr. G. Kimball, of Lowell, Mass., has performed this operation *successfully*.

after the operation, and the consequent strangulation and gangrene, as the cause of death. The intestine was strangulated for five hours after they were discovered, and probably ten or eleven hours in all (*i. e.*, from the time when the patient first began to cough—5 to 7 A. M.), and were all this time indirectly exposed to the air. I accordingly, at 5 P. M., found the protruded portions cold, livid, and covered with an exudation. Probably the vitality was so exhausted already, that no effort was made at reaction of the circulation after the intestine was returned. I had not the least expectation that there would be, at the time. The *post-mortem* appearance went to show that the gangrene extended from the portions of bowel which had been implicated, and thence to the peritoneum; while the peritonitis and the adhesions were principally found to correspond with the incision through the abdominal walls. Considering the state of the patient at the time of reducing the hernia, I did not expect she could recover at all from the shock of that procedure, and the necessary removal and reintroduction of the needles. And yet all this would probably have been avoided had the patient not concealed from me the fact that she had a cough.

4. Finally, I recall the idea of using an *artificial serum* in all cases in which the cavity of the peritoneum is exposed to the air. I have since used it in a case of ovariotomy, and, I think, with advantage. It is merely a common-sense idea that the more nearly the peritoneum is kept in its natural state of moisture and warmth, while exposed to the air, the less is the risk of inflammation of the same. And for this object the artificial serum and the warmth and moisture of the air in the room during the operation, were all deemed important.

Bowdoin College, *March* 1*st*, 1855.

REVIEW.

ART. IX.—*Registration of the Births, Marriages, and Deaths in Massachusetts for the year* 1851. 8vo. pp. 116.
The same for the year 1852. 8vo. pp. 144.
Report of the Secretary of State of Connecticut, relating to the Registration of the Births, Marriages, and Deaths in Connecticut for the year ending August 7, 1848. 8vo. pp. 16.
The same for the year 1848–9. 8vo. pp. 16.
The same for the year 1849–50. 8vo. pp. 28.
The same for the year 1850–51. 8vo. pp. 23.
Report of the Secretary of State of New Jersey of the Registry and Returns of the Births, Marriages, and Deaths in that State, for the year 1851. 8vo. pp. 56.
The same for the year 1852. 8vo. pp. 53.
The same for the year 1853. 8vo. pp. 53.
First Annual Report to the General Assembly of Kentucky, relating to the Registry and Returns of Births, Marriages, and Deaths, from Jan. 1 *to Dec.* 31, 1852.

THREE years ago, in our number for July, 1852, Vol. XXIV., we gave an analysis of the Registration Reports of Massachusetts, for nine years and eight months, ending December 31, 1850, and also of the two reports which had been published by the State of New York, for the years 1847 and 1848; and we then stated that these were all that were then known to have been published on this subject by any of the States of the Union. But here we beg leave to correct our error, and to state that Connecticut had then printed three annual reports, and New Jersey one report; yet neither of these had then reached us, nor did we know of their existence.

Since that time more has been done in those States, and still more is promised. Massachusetts has continued faithful to her undertaking, and sent forth two valuable reports for 1851 and 1852. Connecticut has sent forth other reports, of which we have received only one. New Jersey has given to the world the reports for 1852 and 1853; and Kentucky and Rhode Island have at once leaped into the field, full armed and energized, and published one able and instructive report, and another for the year 1853.

Some other States, as Pennsylvania and New Hampshire, have enacted laws for the establishment of the registration of the births, marriages, and deaths within their borders, and ordered these facts to be reported to, and published by, the duly appointed authorities; yet nothing has been done, and no such reports have been sent abroad from them.

In some other States efforts have been made to persuade their legislatures to establish this system of registration, to procure the records and returns of the three great facts of human life, and their publication by the public authorities.

The National Medical Association has repeatedly recommended this scheme for universal adoption; and we believe that the physicians in almost, if not

quite all the States have, either individually or collectively, urged it upon their legislators, and their men of political influence, to cause it to be done.

Rhode Island has published a valuable report, which has come to hand since this article was prepared for the press. Here, however, is all that is known to have been done by any of the States: twelve reports for twelve years and eight months from Massachusetts; four reports for four years from Connecticut; three or more reports for as many years from New Jersey; two reports from New York; one from Kentucky; and one from Rhode Island.

Several cities have published, and some for many years, their bills of mortality: Lowell, Boston, Roxbury, Providence, New York, Brooklyn, Philadelphia, Baltimore, Washington, Charleston, New Orleans, Memphis.

There may be others which have done this work; but if they have, we have had no information concerning it; certainly the reports of mortality have come to us from no other cities or towns beside these. We have made earnest inquiry of the authorities of some other of our largest and most prosperous cities for their reports of mortality, and have been informed that they have made no such record, although it would have been very desirable.

This work of State registration is almost new in this country, it has taken no great hold of the people; the medical profession have generally given it their support. Statesmen and political economists see the value of it, as developing the law of mortality and aiding in determining the law of population.

The national enumeration of the people, in 1850, included the inquiry as to the number of deaths during the year next preceding the first of June of that year, the sex, age, color, and cause of death of the person dying. The number of the deaths in each State, so far as the marshals learned and reported them, is published in the first volume of the census already printed. But a much more elaborate and minute account of the national mortality of that year is, we understand, in course of preparation in the Census Department, at Washington, and will be published, we trust, in satisfactory tables and statements in the volume which is to appear in the course of this year. In respect to this we will merely stop now to caution the public not to draw any inference as to the rate of mortality from those mortuary tables already printed. Although the inquiry was intended to reach every death in every district, county, and State, during the year proposed, yet the answers and returns are very imperfect; some whole counties made none, others reported half or less than half, so that the number printed in the tables, pages 12 and 140 of the Abstract, and page XII. of the quarto vol., afford no indication of the total number of deaths in the several States and sections of our country, and no confidence whatever can be placed upon any rate of mortality that may be deduced from those statements.

Nevertheless, we are glad of this inquiry, although it developed such imperfect answers, inasmuch as it has called the attention of the people to the subject; and we trust that the errors of the report in regard to the several States, will incite their people and governments to establish their own local systems of registration and reports, and then give such complete returns of their mortality as will be entirely satisfactory to the inquirer and reliable for those who wish to calculate the law of population and human progress by help of these facts.

The work of registration is essentially connected with civilization. The governments of all civilized nations desire to measure their power by counting their people and calculating the amount of wealth, in order to show what degree of physical force they can present for offence or defence, and what means they have of sustaining themselves. A higher state of civilization is

not content with counting the people merely, but is desirous to measure and weigh them, to ascertain not only how many there are, but what is their actual value. It wants to know not only how many there may be living, but how many are sent into life, and how long that life continues.

In this view of the matter, the political economist sees that there is a great difference in the amount of wisdom, power, and happiness that is contributed to the commonwealth, from the child in his forming period, and from the man in his full maturity; and the difference in the amount of good that has been contributed from the infant and the octogenarian is immense. A wise government, then, not only wants to know how many people exist, but how many are born, and how long they live. And beside all this, it wants to know how much that life is worth; what degree of fulness and vigour is, or may be, enjoyed through it.

These matters are determined, in great measure, by the Bills of Mortality, which show at what periods life is terminated, and what are the causes of death. Those governments that count their people do well; those that determine their duration of life, and the causes of death, do better; and those do best of all which search out the obstacles that stand in the way of the most complete development of life, or impair its vigour, or prevent its maintenance, and endeavour to remove the cause of vital depression, which they may thus discover. This belongs to an advanced state of society, in which the ruling powers shall gather, digest, and diffuse that knowledge which will lead the people to the fullest enjoyment of life.

The reasonableness of this system of registration seems so plain, that we had great confidence that, wherever and whenever it should be once begun, it would go on and the progress would be ever onward, and not backward; that every State or government, that should commence this system, would do more and more, obtain more perfect returns, and publish more complete reports year after year.

Unfortunately for this hope, we are compelled to record again the continued failure of New York, which published two annual reports in 1847 and 1848, indicating an interest in the subject, and desire to come up to the requirements of this philosophy, but since that time has done nothing. The law requiring the return of the facts from the cities and towns is not repealed, but it remains a dead letter, and the people do not regard it, nor do the authorities execute it. A law was passed to the same effect in Pennsylvania, in January, 1852, but no report has been made under it, and we understand that some desire its repeal.

New Hampshire enacted a law for this purpose, in 1849, and amended it in 1851, but has done nothing more. No reports have been issued, and, we suppose, no satisfactory number of facts have been reported from the towns to the officers authorized to obtain them.

Connecticut, which published four annual reports for the four years ending 31st July, 1851, has published none since.

Perhaps one ought not to be disappointed; we do not expect that every scheme of improvement will be adopted and executed as soon as it is proposed; nor even that it will always be carried on after it is once put into operation. The system of registration belongs to the highest philanthropy and cultivation. It was proposed at first, not by legislators nor even by statesmen or political economists, but by physicians and others, who were conversant with vital science, familiar with the depreciation of human life, and anxious to ascertain the cause of our short-comings in this respect. In regard to this subject,

these men are in the advance of the lawgivers, and these are in advance of the people. In America, where all power primarily and ultimately rests with the people, it is useless for the legislature to adopt any law unless it is an exponent of the popular will already existing, or finds a response to it, and a support for it, in that popular feeling after it is adopted.

Thus we find the National Medical Association, and the local medical societies, urging this matter upon their several legislatures. In most States their proposition has produced no action of the government, however much some of the wisest statesmen may have it at heart. In some States, as in Pennsylvania, New Hampshire, and perhaps some others, laws have been adopted to this effect, but no fruits have been produced, no returns gathered, or at least no reports published. In one State, at least—New York—the law was enacted and went into operation, and then it stopped for want of popular co-operation. It is plain that, in the last State, the law of the legislature was in the advance of the people, whose sympathies have not yet been sufficiently enlisted to co-operate in the work and report to their Secretary of State, and enable him to give the world the account of their mortality.

Nevertheless, we hope for better things; we believe the seed once sown, however long it may be in germinating—even though it rot and die—will ultimately spring up and send forth fruit.

We now proceed to analyze the reports which we have before us.

There is in Massachusetts, and in the other States, no such officer as Registrar, or Secretary of the Board of Health, to whom the work of registering the births, marriages, and deaths is especially committed, and who is selected for that office on account of his familiarity with the law of population and of mortality, and who has the exact mental discipline and cautious habits of reasoning, which would enable him to discharge the duties of his office in the most perfect manner. But here and elsewhere, this work is made an incidental part of the duty ofsome officer who is selected for other purposes, and has other duties sufficient to occupy all his attention.

In Massachusetts, Connecticut, and New Jersey, this duty is confided to the Secretary of State, and in Kentucky to the Auditor of Accounts. These functionaries are already sufficiently burdened with the legitimate business of their offices, and are necessarily compelled to employ some other person to analyze, digest, and arrange the returns from the local authorities, and prepare the reports of births, marriages and deaths.

In Massachusetts, the Secretary being compelled to employ substitutes, fortunately intrusted the work to able and competent hands. The report for 1851 was prepared by Dr. Josiah Curtis, who had prepared the two previous reports. That for 1852 was prepared by Dr. Nathaniel B. Shurtleff. These two reports, like the eighth and ninth prepared by Dr. Curtis, and the fourth, prepared by Mr. Shattuck, are very valuable and instructive; they are rich storehouses of this class of facts. These now under consideration are very similar to each other, and to those which had immediately preceded them. Their main tables are the same; the deductions and the reasoning alike; but there are the same deficiencies, and we feel, as after reading their predecessors, the same want of more of that fulness of concentrated philosophy and sum of wisdom, that might have been drawn from the whole experience of the preceding years.

There seems to be more and more attention paid to this subject by the people and the local authorities; out of three hundred and twenty-two towns and cities, in 1851, only nine towns, with a population of 11,056, failed to make returns

of deaths; eight with 8,040 inhabitants, and nine with 3,573, made no report of births. In 1852, out of three hundred and twenty-four towns, only five, with 6,834 people, returned no births, and no marriages; and four, with 5,537 inhabitants, reported no marriages. There were manifestly imperfect returns, in 1851 and 1852, of births and deaths from Barnstable and Middleborough, with a population of 10,340. It is worthy of notice, that all these omissions occurred in fifteen towns, which contain only 24,384 inhabitants.

We are not sure that all the other towns made complete returns of all those events that occurred within their borders. Indeed, in some there seems to have been a remarkably low rate of mortality, as well as a very small proportion of marriages and fecundity. Nevertheless, these did not vary so far from the average of their neighbourhoods as to render it expedient to deviate from their report.

From the whole State there were reported—

	Births.	Marriages.	Deaths.
1849	25,773	6,936	20,423
1850	27,664	10,345	16,606
1851	28,681	11,966	18,934
1852	29,802	11,578	18,482

These figures show a progress of attention to the requirements of the law. The increase is in a greater ratio than the increase of population.

The main facts are stated in thirteen tables in the reports of 1851, and in seventeen tables for the year 1852. In these two reports, the first nine tables are alike; the other four differ in some points, and four more are added to the last report.

The first table, which is not numbered, states, in regard to each town, the population in 1850; the number of births of each sex; the number of native and of foreign parentage; the number of marriages of Americans and foreigners; the number of deaths of each sex; the average longevity of those whose age is reported. The same facts are given for each county and for the State. This is the same in both reports.

The second table, numbered I., shows, in each county, the number, both of males and females, born in each month and in the whole year. The parentage of the children born, whether American or foreign, is given for each county. In the same table the plurality cases are, as stated, in the same way, except as to parentage.

Table II. states the same facts as to the stillborn.

Table III. shows the marriages in each county and in each month; and, also, the nativity of the marrying parties in the several counties.

Table IV. shows the approximate age of each of the parties at their first marriage, and in those cases where the male has his first and the female her later marriage, and *vice versa;* and, also, where both parties had been previously married.

The next, V., is a table of mortality, and shows the number of deaths of each sex who died at the several approximate ages in the several counties and in the State.

Table VI. shows the mortality of males and of females in the several counties and at the various periods of life.

In Table VII. is a list of causes of death, alphabetically arranged, and the number of males and females that died from each disease. In connection with these facts, one set of columns states, in regard to the different sexes, the number who died in each month, and another, the deaths at each approxi-

mate age. The whole is condensed and stated as to sexes and months, and, also, sexes and approximate ages.

In Table VIII. the causes of deaths are classified, and the number who died from each cause and each class of causes, and in each county, is given. Thus far the tables have been alike in both of these reports, and the statements similar.

The subsequent tables differ in some points.

In the report for 1851, Table IX. gives the classified list of diseases, and, in connection with this, the deaths from each cause, through nine years and eight months, to December, 1850, and in the year 1851, and the males and females who died in the three years, 1849, 1850, and 1851.

Another set of columns shows the per cent. of deaths from each cause in the same periods, and the same division as to sexes in the three years.

A corresponding table in the report for 1852, in connection with the classified list of diseases, shows the number of deaths from each cause through the whole period of registration, eleven years and eight months. In each of the years 1849, 1850, 1851 and 1852, separately, the total of the four years, average annual mortality of these four years, and the percentage of the whole from all causes that died from each cause in each of these periods, except the total four years.

Table X., for 1851, shows the average age of the persons that died over 20 years old, in each occupation, in the year 1851, and in the seven years and eight months previous.

In 1852, the same is shown in the nine eastern counties and in the five western counties, separately for 1852, and, in the whole State, in the eight years and eight months previously.

In Table XI., of the report of 1851, is a list of the principal towns which made full reports, with their census for 1840 and 1850; the annual average through three years, 1849 to 1851, of the births, marriages, and deaths, and the excess of births over deaths; the rate of all these three events to the population of 1850; and the average age at death during these years. These facts are stated in regard to all the towns which are presumed to have made full reports, and to each county and the State.

In the report for 1852, the corresponding table gives what is called a four years' abstract, showing, in regard to each town and county, and the whole State, the population, male and female, in 1850; the births and deaths, male and female, and marriages for the four years, and the annual average; and the average age at death through these four years, 1849, 1850, 1851, and 1852.

In the report for 1851, Table XII. shows the same facts as were developed in Table IV., in respect to the marriages during the six years and eight months ending December 31, 1850.

Table XIII. is "intended to illustrate the influence of domestic condition on longevity;" showing the average age at death of the unmarried, the married, and the widowed, in each county and the State, through four years.

Table XII., in the report for 1852, gives a four years' abstract of the births; showing the number of males and females who were born in each month, and in each county, and in the State; and also, the parentage, whether American or foreign, of each sex in the several counties. The same is stated in regard to the plurality cases and the stillborn.

Table XIV. makes a statement of the marriages in each county and month, and nativity, whether native or foreign, for the counties, through four years.

Table XV. shows the mortality of males and females in the counties and months, through four years.

Table XVI. shows the number of deaths of males and females, through four years, in the several counties, at the several approximate ages.

These are all the tables in the two reports. They are followed by very valuable and instructive commentaries, extending through over fifty pages in the first, and sixty pages in the second of these reports. These dissertations are worthy of the careful attention of people in every State. The observations in the Report for 1851, give an analysis of the population. The remarks upon the births, marriages, and deaths, the fatality from several causes, the laws of mortality, are faithful to the facts which have been before shown, and illustrate the laws of population and of human progress. A similar testimony must be given to Dr. Shurtleff's admirable observations at the end of the report for 1852, which, although less elaborate, and involving somewhat more error than those of his predecessor, yet they contain so much that is valuable that we would commend them to the careful study of all interested in these matters.

We have before said that these reports are more full and satisfactory than those which had preceded them in Massachusetts, or had come from any other State; but they are not complete. Dr. Curtis and Dr. Shurtleff are compelled to complain of the neglect of a few towns, which, we trust, will be induced, by argument or by the law in future, to make perfect and true records and returns of their births, marriages, and deaths.

In both of these reports, the authors of the observations have assumed, when they made their calculations, that the returns of their facts were complete, and given us the rates of births, marriages, and mortality to the whole people, and not to those from whom the returns were actually received.

In the three years' abstract is the report for 1851, and in the four years' abstract of the report for 1852, the calculations are made for the counties and the State, as if no towns had been omitted. In some of these four years, twenty-three omitted to return the births, twenty-six omitted marriages, and twenty-three omitted deaths, and four more made manifestly imperfect returns of their mortality.

In the four years' abstract, the births in twenty-three towns, the marriages in twenty-six towns, and the deaths in twenty-four towns are left blank, probably not having been returned, or so imperfectly as not to justify their being reported; and the deaths in four other towns which are herein printed, are so far below the average of their neighbourhood or county that they are untrustworthy.

We have calculated the probable number of these events in those towns which made no report, or manifestly imperfect reports, from the averages of other towns adjoining, and of the counties to which they belong, where the population was of similar character, and there probably was a similar proportion of these events. With the aid of this calculation, we have a complete statement of the births, deaths, and marriages in Massachusetts, in the several counties and the whole State during four years.

We have also calculated the population of the counties and the State for 1849, 1851, and 1852, and taken from the census that of 1850, and obtained the average population in each of these four years; and thus we are enabled to give the approximate average number of births, marriages, and deaths, and the annual rates of these events for the years specified.

Average Population of Massachusetts for the Four Years 1849 *to* 1852.

	Average Population.			Area of Sq. miles.	Inhabitants to one mile.
	Males.	Females.	Total.		
Whole State	497,933	516,698	1,014,631	7198.5	140.9
Six sea-coast counties . .	158,064	161,433	319,497	1853.6	172.3
Bristol and Middlesex . .	117,628	125,876	243,504	1285.4	181.7
Five western counties . .	151,172	151,494	302,666	4049.	74.7
Suffolk	71,069	77,895	148,964	10.5	1418.7
State out of Suffolk . .	426,864	438,803	865,667	7188.	120.4

Average Annual Mortality in Massachusetts for Four Years, 1849 *to* 1852.

	Average number of deaths per year.			Rate of mortality.					
				One in			Per cent.		
	Male.	Female.	Total.	Male.	Female.	Total.	Male.	Female.	Total.
Whole State	9,272	9,616	18,888	53.7	53.7	53.7	1.86	1.86	1.86
Six sea-coast counties	2,580	2,727	5,307	61.2	59.2	60.2	1.63	1.69	1.66
Bristol and Middlesex	1,976	2,084	4,060	59.5	60.4	59.9	1.68	1.65	1.66
Five western counties	2,557	2,731	5,288	59.1	55.4	57.2	1.67	1.80	1.74
Suffolk	2,159	2,079	4,233	32.9	37.5	35.1	3.02	2.67	2.84
State out of Suffolk .	7,113	7,542	14,655	59.6	58.1	59.0	1.66	1.72	1.69

Average Annual Births and Marriages in Massachusetts for Four Years, 1849 *to* 1852.

	Births.		Marriages.	
	Number.	Ratio to Population.	Number.	Ratio to Population.
		One in		One in
Whole State	28,588	35.49	9,530	106.4
Six sea-coast counties .	8,711	36.67	2,966	107.7
Bristol and Middlesex .	6,652	36.6	1,784	137.
Five western counties .	7,758	39.2	2,394	127.4
Suffolk	5,567	27.24	2,386	62.43
State out of Suffolk . .	23,121	37.44	7,144	121.1

This table gives us the most and the only complete and reliable rate of mortality that we have seen from any State in this country. It includes the year 1849, when the cholera prevailed to some extent, and other diseases connected with the digestive organs were more than usually fatal. There being 5,577 deaths in that year, against an average of 3,135 from the same causes in the three following years.

The total reported mortality of that year, from all causes, was 20,427, against an average of 18,007 for the years 1850, 1851, and 1852. But, as the mortality of 1849 was greater than the average, so that of 1850 was less, being only 16,606, which was as much below that of the following year as that of 1849 was above it. The average mortality of 1849 and 1850 was 18,514, and that of 1851 and 1852 was 18,713; showing a difference less than the increase of population of 3.3 per cent., which would have given 19,132 as the reported average mortality of 1851 and 1852.

No other State has yet obtained a complete, or so nearly complete, returns of the deaths within its borders as to justify a calculation of its rate of mortality.

There is a difference in the mortality of the sexes in the different divisions of the State, yet a very remarkable and exact coincidence in the whole State. Without doubt there is a difference in the mortality of the sexes in the various ages; one period being more fatal to one, and another to the others. We have neither time nor space to go into this investigation now, yet it is worthy the attention of those who study these matters to look more carefully into the rate of mortality of males and females at the different stages of being.

The various rural districts of the State present some, but not a large, difference of mortality; being the highest, ten deaths among 591 males, and ten among 584 females in the five western counties, the most exclusively agricultural, and ten among 604 females in Bristol and Middlesex, which are near and adjoining the sea-coast, and much engaged in manufacturing, trade, and agriculture; and the lowest, ten in 612 males in the sea-coast counties.

The total mortality of the sea-coast counties was ten in 602, and in the western counties, mostly beyond the highlands, and protected from the east winds, ten in 572. This difference may be due to the difference of social and domestic condition, rather than to the climate, atmospheric or endemic influences. There is a very striking difference between the rate of mortality in Suffolk County, which is composed mostly of the city of Boston, and that of the rest of the rural districts of the State, being in Suffolk ten in 329 males and in 375 females, and ten in 351 of both sexes; whereas, in the rest of the State, these proportions were respectively ten in 596, 581, and 590; showing an excess of 94 per cent. in the rate of mortality of the city over that of the country.

If we compare the twelve cities with the rural districts, we shall find this difference to be still more striking.

We put the facts into a table, to show the comparison for the same period of four years, 1849 to 1852:—

Males.

	Calculated Average Population.	Average Annual Mortality.	Ratio of Mortality.	
			One in	Per cent.
Twelve cities	159,336	4,021	39.62	2.52
Rural districts	336,261	5,251	64.03	1.56
Females.				
Twelve cities	130,437	3,983	45.3	2.207
Rural districts	338,597	5,633	60.1	1.66
Both Sexes.				
Twelve cities	339,773	8,004	42.45	2.35
Rural districts	674,858	10,184	62.00	1.61

Births.

	Number.	One in	Per cent.
Twelve cities	10,999	30.9	3.23
Rural districts	17,589	38.36	2.60
Marriages.			
Twelve cities	4,920	68.	1.45
Rural districts	4,611	146.3	.68

Thus we see that in the different sexes as often as 100 die in the country districts, 161 males and 133 females, and 155 of both sexes die in the cities.

It is well known that the composition of the population differs in these dense and sparse towns. In the former the population is progressive, growing in some of them rapidly.

It is a matter of deep regret that in the volume of the national census, recently published, the abstract of ages is not extended to towns and cities, but is limited to counties. We have no legitimate means of determining the relative ages of the population in the city and country. It is admitted, as a general fact, that there is a difference, and that the population is older where it is scattered and engaged in agriculture, than when it is condensed and occupied in commerce and manufactures. Although this is not now demonstrated in regard to the cities of Massachusetts, yet the statements of the former censuses in regard to these and other cities of the United States, and the analogy of cities in other countries, will justify us in assuming that there are in these cities a greater proportion of people in the younger and middle ages of life—that the population is generally more active, and there are more inducements for marriage in the compact towns than in the country. This is seen by the comparative ratio of marriages and of births. Recurring to the preceding table, we find that, among 10,000 population, the marriages in the country were 68, and in the city 145. The births were 260 in the country, and 323 in the city; that is, as often as 100 were married, and 100 born in the rural districts, 213 were married and 124 were born in the compact towns. This, of course, produces a larger proportion of infants and children, who would increase the rate of mortality, without diminishing the expectation of life.

There is another matter of consideration in the different original elements of population in the cities and country of Massachusetts. In the former is a very much greater proportion of foreigners, especially the Irish, than in the latter. This proportion is greater in Boston than in other cities. Dr. Chickering, in his report on the population and mortality of Boston, states that, in 1850, there were 75,466 Americans and their children, and 63,466 foreigners and their children, in that city. The mortality, through four years was, of Americans, 9,754, and of foreigners and their children, 11,816. The marriages were, in three years, of Americans, 3,932, and of foreigners, 4,643. Births in four years, of Americans, 7,153; foreigners, 13,536. Thus, while there were only 82 foreigners for every 100 American inhabitants, there were 121 deaths, 118 marriages, and 189 births among the former, for every 100 among the latter; showing an enormous disproportionate excess of these events among the foreign population of Boston during the years here noticed.

In 1850, the foreigners constitued 16.5 per cent. of the whole population of the State. From the first table in these reports, we are enabled to distinguish the parentage of those who were married, and of the children who were born, in the years 1850, 1851, and 1852. In these years the births were, of American parentage, 46,384, or 56.2 per cent.; and of foreign parentage, 36,342, or 43.8 per cent. of the whole. The marriages were, American, 22,421, or 68 per cent., and foreign, 10,291, or 31.4 per cent. of all. Thus, while in 1850, for every 1000 natives there were only 199 foreigners, there were in that and the two following years, for every 1000 children born in American families, 457 born in foreign families; and for every 1000 marriages of natives, 778 marriages of aliens.

In this connection it should also be considered that a larger proportion of the foreign immigrants are of the marriageable age, between 20 and 40 years

of age, than is found in any fixed and stationary population; they would, therefore, have a greater ratio of marriages and births than the natives until their first generation shall have passed away.

These foreigners are generally among the poor, and belong to the labouring classes. They perform the hardest work, and are exposed to the greatest dangers of the elements, heat, cold, and wet. They, in the cities especially, live in crowded and unventilated houses and rooms. They are addicted to unhealthy habits of drinking and smoking tobacco. They are subject to many privations, and exposed to many causes of disease more than the natives. Their children have less chance of life, and they are supposed to be subject to earlier death than the Americans.

In calculating the rate of mortality these facts should be considered, and, in determining the expectation of life in any State, this distinction should be made between the foreigners and the natives, who have such widely different habits and such different elements of vitality.

"No table has been prepared to show the relative deaths of natives and those of foreign birth. It seems desirable that, in future abstracts, this element should be regarded, as the subject is one that is now awakening considerable interest among those engaged in the study of the statistics of deaths. A matter of much importance to this class of inquirers would be to ascertain the proportion which exists in this Commonwealth between the deaths of children of American and foreign parents. There is good reason to believe that the early mortality of the offspring of our own countrymen is much exceeded by that of the children of immigrant parents, especially in the large cities and thickly inhabited towns. There is very little doubt that the duration of life in this community is less with the Celt than with those of Teutonic blood, and that everywhere the expectation of life is lower among the needy and destitute, with those poorly clad, scantily fed, and living hived together in underground and unventilated apartments, than it is with the prosperous and well-conditioned."—*Eleventh Report*, p. 112.

In the report for 1852, Dr. Shurtleff gives a very valuable analysis of several diseases—cholera infantum, dysentery, erysipelas, typhus fever, scarlatina, scrofula, consumption, and teething—showing, in regard to each, the annual total mortality in the years 1849, 1850, 1851, and 1852, and the ratio of the whole to the population of each county, the number and proportion dying at each age, and the males and females dying in each month of each year.

These are very important tables, and we hope they will be continued and extended to all other diseases in the future reports. The ratio of mortality from these causes to the population in several counties must be taken with some limitation, inasmuch as several towns made no report, and yet their population is included in the calculation. In so valuable a document as this, which ought to be worthy of all and unhesitating credence, this should have been regarded; and either the mortality from these diseases in the towns of Barnstable, Holyoke, &c., which did not make reports, should have been calculated from the analogy of their respective neighbourhoods or counties, or their population should have been omitted. In either way, the rate of mortality would have been raised and brought nearer the facts. In the county of Barnstable, the ratio of deaths from dysentery would be one in 128, not one in 166 of the people, as here stated. This is the most marked error, as a larger proportion of this county than of any other failed to make the required returns.

Our author reasonably complains of the want of uniformity in the diagnosis

and nomenclature of diseases in the returns which came from the town and city clerks. Some names seem to be more readily used in some counties than in others. Thus cholera infantum, nominally, at least, was much more prevalent in Middlesex, where one in 473 perished from it, than in Berkshire, where it destroyed less than one in over 4,000. Probably some cases which are called by this name in one place are called diarrhœa or bowel complaint, or other name, in another place. Thus, Middlesex, which has so much higher proportion of cholera morbus, has a somewhat lower proportion of other diseases of the digestive organs than Berkshire and Franklin. There seems to have been a similar uncertainty in the use of the term consumption, as 1,386, or 9 per cent. of the whole who perished from it in the four years, were under five years of age. It is hardly probable that their disease was tuberculous phthisis.

The *Reports from Connecticut* are more meagre than those of Massachusetts. We have four of these documents for the years ending with July, 1848, 1849, 1850, and 1851.

These four reports are prepared by, or under the charge of, three different Secretaries of State, and there is no evidence that they were submitted to a competent physician or statistician for preparation or correction. They are made up from the returns as they come from the several districts and towns.

These returns to the secretary were very imperfect. He complained, in the first report, that the law did not allow sufficient time to prepare and send the blanks from the seat of Government to the school committees before some of them had made their customary enumeration of the children, and "they were unwilling to undertake the labour of canvassing their districts a second time, and either made partial and imperfect reports, or wholly neglected the required returns." "Under such circumstances, full and accurate returns were hardly to be looked for." "From thirteen towns no returns were received, and in several others only a small proportion of the school districts made the required reports."

The sum of these reports is shown in the following table, by which we see that there was less progress of improvement in these four years than was reasonably expected:—

Years.	Births.	Marriages.	Deaths.
1847–8	6,850	2,816	4,379
1848–9	7,238	2,920	5,000
1849–50	7,578	2,884	5,118
1850–51	8,360	2,995	4,712

The first report was hastily made from facts hastily gathered, as the law went into operation on the 27th of June, and required the records to be made covering the whole year, ending on the 7th of August next, and then to be returned early to the Secretary of State. The secretary comprised the facts which he received in three tables.

Table I. shows, in regard to each town and county, the population according to the preceding census; the births and deaths, male and female; the marriages in three classes: 1st, both parties resident of the State; 2d, but one resident; 3d, both non-residents; a recapitulation in regard to each county and the State.

Table II. shows, in regard to each county and the State, the mortality, male and female, in each month.

Table III. shows, in regard to each county, the deaths of males and females, at their several ages.

A new secretary does the work of the second report. In this there is an increase of the returns of all the facts, yet the secretary complains of the remissness of the local officers in making the proper inquiries and sending their results to him.

The facts that were obtained are published in four tables.

Tables I. and III. are similar to those in the first report.

Table II. shows the number of births, male and female, in each county and in the several months.

Table IV. exhibits the number of deaths in each county, from the several causes, alphabetically arranged.

In the third report, the third secretary echoes the complaint of his predecessors: "While the returns, on the whole, are somewhat more full than those of the two preceding years, they are yet far from complete, and, in many of the particulars which they were designed to embrace, extremely defective." "From two towns no returns were received;" yet, he adds, "In our own State there are indications of awakening in this subject, and of increasing convictions of the important benefits to be derived from its prosecution."

The facts received are condensed into five tables.

Tables I., II., III. and IV. are similar to those in the previous report, except that the diseases are classified.

Table V. shows the mortality from each class of causes, and also, the proportion in each class to the whole from all specified causes, in each of the last three registered years.

The fourth report is prepared by the same secretary that prepared its predecessor. He still complains of the neglect of the local committees in making returns. He refers to the delinquency of some of these officers in sixty-two towns, and the total omission in two others. Nevertheless there was in the returns an increase of births and marriages, and a decrease of deaths.

He publishes his facts in five tables, all of which are similar to those in the third report.

These facts in all these reports are valuable and reliable as far as they go, but they are incomplete, and allow no ground to calculate the rate of births, marriages, or mortality to the population of the State. They are published almost without note or comment, naked statements for the statistician and the political philosopher to clothe and use for such purposes as they may answer. The nomenclature and classification are those proposed by the National Medical Association. From this we learn, at least, the relative prevalence of each disease, or class of diseases, in Connecticut, although we cannot determine the absolute amount of its influence.

It was an unfortunate direction of the law that required the school committees to gather these facts in Connecticut. The same was tried without success by Massachusetts during the first few years of this law, but a charge from the school committee to the town clerk or register produced a great improvement in the returns. The school committees are elected, not so much for their general business habits as for their interest in, and acquaintance with, education. They are willing to attend faithfully to the main object of their office, but go unwillingly beyond it, and, as we see in both Connecticut and Massachusetts, they have often come short of this unacceptable requirement. We hope Connecticut will take counsel of her sister State, and thus improve her returns and reports of mortality.

The law of *Registration in New Jersey* was enacted in 1848. It required the ministers, justices, and all others authorized to join in marriage, to report

such acts, and sextons to report all burials to the township clerks, and the clerks themselves to make inquiry as to births, record these facts with the several circumstances specified in the law, and send the whole record to the Secretary of State. In 1851, a supplement to the law increased the compensation to the clerks and the secretary for the work required of them.

We have not been able to obtain the earliest report, but we have the last three, covering the years 1851, 1852, and 1853.

In the report to the legislature, the secretary, who is the same through these three years, complains of the remissness of the local officers, and urges the legislature to clothe him with some authority to compel their obedience to the law. He says, in the report, printed in 1852: "General complaint is made by the township clerks that ministers, magistrates, sextons, and physicians do not make report to them as directed by the law. Physicians are especially, and almost universally, spoken of as remiss in this particular. A penalty for such neglect is affixed to the three former classes. If a penalty is proper in any case, *physicians ought to be included.*"

In the following report, printed in 1853 and 1854, the secretary repeats his complaint and recommendation of more stringency in the law.

Out of one hundred and eighty-four townships in the State, ninety-three sent returns for 1850, one hundred and fifty-six for 1851, one hundred and fifty-seven for 1852, and one hundred and forty-one for 1853.

The facts returned to the secretary are condensed in the following table:—

Years.	Births.	Marriages.	Deaths.
1851	11,861	2,859	5,089
1852	10,683	2,682	6,371
1853	12,107	3,388	5,651
	34,651	8,929	17,111

These are stated in seven similar tables in all the reports; showing, in respect to each town and county, and the whole State, the number of deaths from the several causes.

The number of deaths in several decennial periods of life.

The number of male and female children born, and the general occupation of the father.

The number of marriages.

These facts are published without note or comment, leaving it for the statistician to make what deductions he may from them.

The nosology is a limited one; it includes only twenty-one diseases in the reports for 1852 and 1853, and twenty-three in that for 1851.

We are not to suppose that these were the only fatal diseases in New Jersey during these three years; for there is another large and ominous class, one very convenient to the reporters, but very inconvenient to the analyzers, under the name of "all others." There were 17,111 deaths reported, and of these the causes of 14,323 were given; and of those whose diseases were returned, 3,069, or more than 21 per cent. were placed by those who prepared the report in this class of "all others."

We have read and examined the *Report from Kentucky* with great satisfaction. This is the first report which that State has made. They have not there minute local divisions into small towns and municipalities, with the local officers, select-men, town-clerks, committee-men everywhere; they have only counties for divisions, and only the county courts or assessors to make the inquiry. It is, therefore, a matter of congratulation that their first report

is so full, so nearly complete, and so instructive. Great credit is due to the officers who, throughout that great State, gathered all these facts, and especially to Dr. W. L. Sutton, of Georgetown, Ky., who prepared the report, arranged his facts in well-digested tables, and sent them forth with the wise observations that followed them. We commend this example of Kentucky to other States that are doubting whether they shall begin or pursue this all-important inquiry into their vital and mortuary condition.

This report of Kentucky fills one hundred and twelve pages. There were received from the assessors in the counties and published in this report the record of—

Births, 25,906; marriages, 7,430; deaths, 13,048.

The facts are put forth in ten tables; showing, in respect to each of the one hundred and one counties, the population, white and coloured, separately; the births of each sex, white and coloured, separately; the deaths of each sex and colour, and proportion of births and deaths, white and black, to the whole population of their respective colours; the average age of the whites and the coloured at death, and the number of marriages. The last are given in two columns; one furnished by the county clerks, which shows the number of licenses issued, and the other gathered by the assessors.

Table II. shows the number of each sex born in the several counties during each month.

Tables III. and IV. show the same facts in respect to the plurality cases and the stillborn.

Table V. shows the marriages in the several counties in each month.

Table VI. shows the ages of the marrying parties in the cases separately; 1st, when it was the first marriage of both; 2d, when it was the first of the female and a later one of the male; 3d, when it was the first of the male and a later one of the female; and 4th, when it was a later marriage of both.

The numbers and sexes of those who died in the respective counties are shown, in respect to each month, in Table VII., and, in respect to each age, in Table VIII.

Table IX. is a list of the causes of death, alphabetically arranged. This nosology is, with few exceptions, the same as that proposed by the American Medical Association and adopted by Massachusetts.

Against each disease is shown the number of deaths, of whites and of blacks, of each sex; the number who died in each month, and of those who died at each period of life.

Table X., A shows the number who died from each of the twelve classes of causes of disease in the several counties. B shows the number who died in the State and in each county from each specified cause, and also, the proportion which the mortality from each disease bore to the whole throughout the State.

Here is, indeed, a very comprehensive list of tables and of facts, and a work of great value to those who wish to study the law and progress of population and the sanitary condition of Kentucky, and who desire to compare its diseases, and some of the circumstances of its mortality, with those of other States. Kentucky, although it has no great variety of latitude, yet it is widely extended from east to west, and has a great variety of soil and geological structure. The mountain regions of the east are probably subject to very different endemic influences from those of the alluvial, and, in some places, low districts of the west. The counties are small, and enable the student to discriminate very nearly the various localities. The diseases of each county are

specified; and thus the connection between these localities and diseases and the effect of local influences can be traced, and, in some measure, determined.

These returns, although so full and gratifying, are not complete. One county, with 7,393 inhabitants, returned no deaths. Ten other counties, with 5,095 coloured inhabitants, reported no deaths of that race. All the other counties show some mortality both of the whites and of the blacks, yet some of these are evidently imperfect. Jefferson County, which in 1850 contained near 60,000 people, and included the city of Louisville, with a population of 50,000, returned only 390 births and 205 deaths of all colours. There are only about one-fifth of the number that is stated by the marshal, who took the census in 1850, as having been born or died in that county in course of the preceding year. Two years and a half had elapsed, and the population considerably increased and undoubtedly added to the number of births and deaths.

The manifest incompleteness of these returns in many of the counties allows of no calculation of the rate of mortality, or of births or marriages in them, or in the whole State. It might have been very properly made in regard to those counties whose returns bore evidence to Dr. Sutton, from his knowledge of their circumstances, that they contained all these facts that had happened within them. These would have been satisfactory; the others are at least doubtful, and some of them plainly erroneous. We were, therefore, sorry to see the calculation made and the result stated, in the first table of this report, of the rates of births and of deaths of each colour, to the white and the black population in each of the counties, which reported even only one of these events.

The ratio of marriages to the population is more reliable; for those are generally taken from the registers of the county clerks, whose license is necessary for the legality of this act. But the enumeration of the births and deaths had no such ground of certainty. They were gathered as the assessors had power or will to obtain them, or as the people of whom they inquired were able or willing to give the information. There is a great difference in the facts reported, and consequently a corresponding difference in the rates deduced, in the separate counties. Among the whites there is stated to have been one death in 687 in Muhlenburg, one in 423 in Floyd, and all intermediate rates up to one in 321 in Hickman. Among the blacks, this rate is one in 1,152 in Lincoln, one in 605 in Adair, up to one in 17 in Marshall. In thirty-two counties the mortality was less than one per cent., and in six counties less than one-half per cent., among the whites; and in twenty counties less than one per cent., and in seven counties less than one-half per cent., among the blacks. The average rate of mortality was one in 78.28 whites, and one in 65.07 blacks. Now, no one acquainted with the law of mortality will put any confidence in these deductions.

The statements of the births bear more internal evidence of completeness, yet they do not seem to justify the calculations which are here given of the rates of the fecundity of the different colours in the several counties. It is not to be supposed that there should be such a wide difference as one birth in 22 people in Butler, Hancock, Laurel, and Marshall, and one in 188 in Jefferson, and one in 317 in Muhlenburg among the whites; and one in 13.1 in Marshall, one in 21.5 in Warren, one in 22 in Shelby, and one in 259 in Muhlenburg among the blacks; or that in Caldwell there should have been one birth in 36 whites and one in 157 blacks; in Greenup, one in 45 whites and one in 164 blacks; in Perry, one in 21 whites and one in 120 blacks. Between these extremes there are all intermediate grades, owing to the various and defective returns.

This defect is what should have been expected. The same has been the

experience in all other States in their first attempts to gather these facts. Indeed, in Kentucky it seems to have been less than many other States. These returns seem to come much nearer the actual facts than any of the four reports of Connecticut, the last three of New Jersey, the two of New York, or the first of Massachusetts and England. Great credit is due to the assessors of Kentucky, who wrought so well in a field entirely new, and to Dr. Sutton for the faithful and successful labours he has performed; and we hope he will be again and again intrusted with the same work until it shall be brought to perfection.

Having given a general account of those reports from these four States, we would now compare their facts and the condition they represent, so far as they have principles or facts in common.

The relative prevalence of the various causes of death and the degree of mortality, is a matter of the first interest. Unfortunately for this comparison, the reports of Massachusetts alone make sufficient approach to completeness to allow a calculation of these rates to the numbers of the people. The nosology of New Jersey is so meagre that we have no great confidence that the numbers stated to have died from the several classes of disease contain the whole, for we know not how many fell into that receptacle of small numbers, "all others." Nevertheless, with this caution to our readers, to make all due allowance for what may have fallen therein in New Jersey, we give the following table of the mortality, and the comparison of the relative prevalence of the various classes of disease, and of several of the most important specific causes of death in the four States now under consideration:—

So far as this limited observation justifies any inferences as to the relative prevalence of the various causes of death, it appears that it arises from the diseases of the endemic, epidemic, and contagious class, in Massachusetts, in a proportion a little more than half as large; and in New Jersey less than half as large as in Kentucky. The proportion of diseases of the nervous system is, in Massachusetts, greater; and in Connecticut, sixty-five per cent. greater than in Kentucky. There is an observable difference in the relative fatality of diseases of the lungs. The proportion of consumption is nearly equal in all the three eastern States, and more than twice as great in these as in the western State. But the proportion of every other pulmonary disease, except of pleurisy, which was equal, was higher in Kentucky than in Massachusetts. Pneumonia, scarlatina, and croup, show a much higher ratio in the former than in the latter State. A similar difference is manifested in regard to asthma, bronchitis, hydrothorax, laryngitis, quinsy, and the unspecified diseases of these organs. Deducting consumption, the result shows that of all other diseases of the respirative organs, 1,585 died in Kentucky, and 915 in Massachusetts, out of 10,000 deaths from all reported causes; and the whole burden of pulmonary diseases was relatively not so much greater in the eastern than in the western State, as the burden of the diseases of the digestive organs was less.

There was a marked excess of ratio of dysentery, cholera, and fevers in Kentucky, over those in the other States. The ratio of deaths from old age was greater in Massachusetts and Connecticut than in Kentucky; but there was a larger class in which these deaths should take place. According to the census of 1850, the proportion of persons over 60 years old to the 10,000 of all ages was, in Massachusetts, 610; Connecticut, 773; and Kentucky, 357. If the ratio of mortality was the same as this in all, the deaths from old age would be in the ratio of 170, 216, and 100 in these States respectively;

whereas, they are in the ratio of 260, 305, and 100, indicating the probability, at least, that the warmer region is less unfavourable to the advanced period of life than the colder.

The deaths by violence include accidents of all sorts, from railroads, machinery, steam, drowning, &c. as well as personal assaults and murders.

From the tables of mortality in the several months, in the reports from Kentucky, Connecticut, and Massachusetts, we are enabled to form some estimate of the comparative fatality of the several seasons in those States. Our calculation includes one year in the first, and four years in the latter two. The order of mortality, in the several months, differs very materially among them. Beginning with the least fatal, it was, in Kentucky: February, January, May, April, March, June, November, December, October July, September, August. In Connecticut: December, November, October, September, January, August, February, June, April, May, March, July. In Massachusetts: June, November, February, May, January, April, December, July, March, October, August, September.

The warm season, in all of these States, was more fatal than the cold; there was the greatest difference in the months in Kentucky. Making corrections for the difference of length, there were, in that State, 150 per cent. more of deaths in August; and in Massachusetts 61 per cent. more in September than in February; and in Connecticut 55 per cent. more in July than in December.

Comparative Mortality of the different Seasons. Ratio of 10,000 *Deaths through the Year.*

	Kentucky. One year, 1852.	Connecticut. Four years, 1848 to 1851.	Massachusetts. Four years, 1849 to 1852.
Fourth quarter, Oct., Nov., Dec. . .	2,488	2,170	2,340
First quarter, Jan., Feb., March . .	1,760	2,589	2,230
Second quarter, April, May, June .	1,858	2,658	2,105
Third quarter, July, Aug., Sept. . .	3,823	2,580	3,324
Cold season, fourth and first quarters	4,269	4,760	4,560
Warm season, second and third q'rs.	5,750	5,239	5,529
Winter, Dec., Jan., Feb.	1,882	2,333	2,177
Summer, June, July, Aug.	4,284	2,704	2,701

The mortality at different periods of life is shown in these reports. It is not important here to state this at all the shorter quinquennial or decennial periods, but we are content with showing this at the great epochs, as in infancy, under 5; in the forming and developing period, 5 to 20; the active and responsible period, between 20 and 60, when man assumes the burdens and bears almost the whole responsibilities of life; old age, 60 to 80; and extreme old age, 80. As the whole value of this statement rests upon the ratio of the mortality in each epoch to the number living in that epoch, in the State observed, we have calculated the ratio per 10,000, living at each of those periods in these States.

Comparative Mortality at various Ages in different States. Ratio of Population and Deaths, at each period, per 10,000 *of all Ages.*

AGES.	MASSACHUSETTS.		CONNECTICUT.		NEW JERSEY.		KENTUCKY.	
	Ratio of population, 1850.	Ratio of mortality, 4 years.	Ratio of population, 1850.	Ratio of mortality, 4 years.	Ratio of population, 1850.	Ratio of mortality, 3 years.	Ratio of population, 1850.	Ratio of mortality, 1 year.
Under 5	1,238	3,601	1,367	2,368	1,327	4,246	1,293	4,030
5 to 20	2,970	1,108	2,092	1,619	3,390	1,212	2,943	2,057
20 to 60	5,181	3,299	5,765	3,409	4,794	2,874	5,406	2,941
60 to 80	546	1,366	684	1,740	439	1,207	321	731
Over 80	64	682	89	856	50	477	36	250

If the returns included the entire mortality of these States, it would be easy at once to determine the ratio of deaths to the living at every epoch in each, but the returns are incomplete in all. Supposing, then, that those which we have represent the ages of the others which we have not, we assume that the whole mortality of each State was distributed through the several epochs of life, as these are. We can then see, whether this distribution of deaths, through the several ages, bears the same relation to the distribution of the living in all of those States.

In this way we see that the burthen of mortality in infancy was the least in Connecticut, greater in Massachusetts, and almost twice as great in New Jersey and Kentucky. In youth, this burthen is nearly equal in Massachusetts and New Jersey, and greater in these than in Connecticut and Kentucky, in which it is nearly equal. In the period of active manhood, the burden rests more lightly on New Jersey and Kentucky than in the other States, in which it is not very unlike.

We have not much confidence in the value of deductions drawn from the average age at death, except when it is taken from large numbers and in connection with the ages of the living. In a growing population, the families are younger and children more abundant than in a stationary community, which has a larger proportion of persons in the advanced periods of life. The children and youth have the same liability to death in both; and, as there are more of the young and fewer of the old to die in the former, its bills of mortality must show a lower average longevity than the other. For this reason, most American cities, which are growing rapidly and have a younger population, present a lower average age than the rural districts whose living population is stationary and older.

The calculation of the average longevity increases in value with the number of persons who are included in the observation; and it is worthless if it includes only a small number. Accident may make a great difference in this average and the apparent expectation of life in the district, or the class from which the calculations are made.

The Kentucky report states that the average age at death of the blacks in Morgan County was *one day*, and in Lincoln thirteen days. On looking to other columns we find that in each of these counties there was only one death. If in next year the child's grandmother should die, the average may be raised from one day to eighty years; showing a great apparent increase of healthfulness and expectation of life in the district. In Clinton, the average of the blacks was forty-five years, and based upon the same extent of observation—a single death.

Comparative Mortality from different Causes. Ratio per 10,000 *from all specified causes in each State.*

	Massachusetts. Eleven years and eight mos., May, 1841, to Dec. 1852.	Connecticut. Three years, July, 1848, to July, 1851.	New Jersey. Three years, 1851-52 and 1853.	Kentucky. One year, 1850.
Specified causes	138,550	12,827	15,638	10,411
Ratio	10,000	10,000	10,000	10,000
All specified causes.				
Zymotic diseases	2,937	3,232	2,506	5,643
Sporadic diseases				
Uncertain seat	1,217	763		578
Nervous organs	969	1,106	885	669
Respiratory organs	2,794	2,807	2,406	1,727
Circulative organs	208	152		68
Digestive organs	655	408	223	439
Urinary organs	44	70		35
Generative organs	114	149		125
Locomotive organs	55	61		55
Integumentary organs	9	17		15
Old age	612	717	577	235
Violence	386	513		420
All diseases of lungs	3,144	3,132	2,781	2,505
All diseases of digestion	1,858	2,924	1,322	3,208
Consumption	2,229	2,132	2,023	920
Pneumonia	441	520	282	579
Fevers	717	750	259	1,547
Dysentery	787	1,253	1,031	1,839
Scarlatina	411	255	462	104
Measles	104	82	110	318
Cholera and cholera infantum	316	271	65	824
Croup	217	210	258	443

In the last Massachusetts report a similar variance is shown in the average age of persons dying in some of the occupations. The average longevity of railroad agents and conductors in the western counties was twenty-four years, and in the eastern counties forty-six years. Artists in one district died at the age of fifty-one, and in another at twenty-seven. Coppersmiths lived in one part of the State to their fiftieth year, and in the other only to their twenty-third.

No possible good can be derived from such statements as these. No inference can be drawn as to the healthfulness of the places or occupations, or the value of life among those who reside or are engaged in them. On the contrary, they may mislead those who look only to these columns, and not to the facts on which they are founded, or who are not used to this kind of investigation; for they may be thereby induced to determine their location or employment.

The average age of death is calculated in Massachusetts for the last four reported years, and stated as to each town and county and as to the whole State. We again express our regret that this calculation was not extended to the whole experience of eleven years and eight months. The whole facts have been reported and are in the office of the secretary, and they would have given a very greatly increased value of these averages. And when so much might have been done, and so much added to the truths of vital science, it is

a matter of pain and wonder that the opportunity was neglected, and a comparatively inferior report sent forth to the world.

This calculation includes 72,715 deaths in Massachusetts. In the State, and perhaps in the counties, it gives a fair average of the duration of life; but in many of the towns, probably, a wider and larger observation would vary the result. The longevity is the highest in the small agricultural towns, which, beside being the most favourable to health, have a comparatively stationary population generally engaged in agriculture, the most healthful employment. The excess of their youth go elsewhere to find other fields of occupation, and none come in. Some of them present a high average longevity. In Chilmark, with 36 deaths, it was 57 years; in Burlington, with 32 deaths, it was 52 years; in Medfield, with 73 deaths, it was 51 years; in Dalton, with 39 deaths, 50 years. In fourteen towns the averages were over 45 years, and in fifty-four towns over 40 years. On the contrary, there were ninety-four towns whose average was under 30; thirty-three under 25; and five under 20. All of the last, and most of the last but one of these classes, were places of recent and sudden growth and seats of manufactures.

The averages of the fourteen counties were, respectively, 20, 25, 26, 27; three counties 29, 31, 32, 33, 34, 35 and 39 years and a fraction. The lowest was Suffolk, whose population is rapidly increasing, with a large proportion of young Irish families among them. The next was Middlesex, which has three growing cities and many large manufacturing establishments. The average longevity of the whole State was 27.02 years.

The average in Kentucky was, among the whites in four counties, over 30 years; in twelve counties, 25 to 30 years; in thirty-four counties, 20 to 25 years; in forty-six counties, 10 to 20 years; and in three counties, under 10. Two were not reported.

Among the blacks in one county was one death at 45, and, in another, 146 deaths at the average of 35. In the other counties the averages were in six over 25 years; in twenty, 20 to 25 years; in fifty-two, 10 to 20 years; and in eight, under 10 years. Thirteen were not reported. In the whole State, 9,631 whites died at an average age of 20.55 years, and 3,367 blacks at an average of 17.59 years.

We hope Dr. Sutton, in the coming reports, will add the experience of future years to that of 1852; and, probably, as the State grows older and more cultivated, the average longevity will be very materially increased.

The average age is not stated in the reports from New Jersey and Connecticut, nor do we think it now worth our while to calculate them.

Occupation.—The reports from Massachusetts have, from the beginning, given tables of the longevity of those who were engaged in the several occupations, and died at an age of 20 years and over. We have already, in our *Journal* for July, 1852, spoken of the defects of this table, of the minute division, and of the want of concentration of the whole sum of the observations in the State, and of the bad classification into groups of occupations which are dissimilar in character and sanitary influence. Here we have occasion again to utter our complaint. We do it with sorrow; for, where there is so much that is satisfactory, it is not agreeable to find fault; but, in the report for 1851, the former defects are repeated, and, in that for 1852, the same are continued and another is added.

In the tenth report, the number, sum of ages, and average age at death of those who belonged to the several occupations, are given—not for the whole ten years and eight months—the record of which, in regard to the males, had been received; but for seven years and eight months, ending with 1850 in one class, and for the year 1851 in another, and these are not added together.

In the eleventh report, the statements, including the deaths in the whole State during the eight years and eight months, ending with 1851, are in one column, and those who died in the year 1852 are in two other separate columns; one covering the five western counties, and the other including the nine eastern counties. We cannot understand why this last division was made. We know of no difference in the circumstances of life and the sanitary exposures of a blacksmith or shoemaker in Middlesex or Essex, and a blacksmith or shoemaker in Worcester or Berkshire.

This division of occupations is not only unscientific, but needless; for, in many of these, the numbers of deaths were already so small as to render their average of no value. In these cases, the accident of a few of the young or old dying in different places determine very wide and singular differences in the statements of the value of life among them. Thus, the average longevity of the bakers was, in the eastern part of the State, 42.82 years, and, in the west, only 26. In the east, the paper-makers lived 39.23 years, and, in the west, only 21.

The average is stated in regard to each occupation, which is well. But, inasmuch as many of them present but few deaths, and of course a very variable and unreliable average of longevity, they need to be condensed into classes, according to the affinities of their circumstances or their effect on health.

Our authors make eight classes of the males and one of the females. These are: 1, agriculturists; 2, labourers; 3, mechanics; 4, merchants; 5, paupers; 6, professional men; 7, public men; 8, seamen; 9, females.

The names of all these classes, except the seventh, probably explain them. But this class of public men is one of the anomalies of science. We have in vain sought to find some law of affinity, some vein or bond of common character or circumstance running through or surrounding them, which should unite them together and separate them from others. We give their names: Baggage-masters, bank-officers, brakemen, brokers, chimney-sweepers, comedians, cooks, drivers, drovers, engineers and firemen, express-men, fencing-masters, ferrymen, gentlemen, innkeepers, judges and justices, lighthouse-keepers, news-carriers, pilots, public officers, railroad agents and conductors, sextons, sheriffs and constables, soldiers, stablers, teamsters, ticket-masters, victuallers, watchmen, weighers and gaugers. Perhaps they are all, in some sense or other, public men; that is, they either appear abroad before the public, or the public has the privilege of coming to them in the way of their employment. In this respect, the broker and the merchant, the pedler and the teamster, the ferryman and the labourer, and, indeed, most of the others in this and some in the other classes are in the same category. Yet there are some whose occupation neither call them to the public nor invite the public to them; such as cooks and chimney-sweepers. Even admitting that these had no affinities with, and must be separated from others, yet there is, at least, an unnatural juxtaposition of occupations that have nothing but mere humanity in common, and whose exposures and influences are as dissimilar as possible. What affinity there is between the judge, whose labour is all of the brain, and the chimney-sweeper, whose work is all of the muscle; between the gentleman, who is usually a capitalist, without special occupation, and the stabler, whose occupation is among horses, harness, and filth; between the cook, who lives over the fire, and always hot, and the pilot and teamster, whose life is necessarily abroad, and often in the cold and storm; between the broker, who lives by calculation, amid the anxieties and fluctuations of the money-market, and who has a life of physical ease, and generally of inaction and great comfort, and the watchman, whose labour is at night, and who generally adds to this as

much work in the day as he can endure; what similarity there is between these and other occupations of this class, in the sanitary influences of their employments, or of the circumstances which surround them, in the exposures to which they submit, in the power of self-protection, in the comforts they may enjoy, in the effect of their various kinds of cultivation, habits, and position upon human health and constitution; these matters we are unable to understand, and respectfully ask the authors of these otherwise valuable documents, to explain them in their future reports.

The paupers are included in a single and separate profession, and form a class by themselves, and they have a very high longevity, the highest of the whole. Truly pauperism seems to be a very healthy occupation; and the poor appear to have at least the blessing of length of life, as a compensation for their worldly privations. This would be a very pleasant consolation to these people in their sufferings if it were true. On the contrary, the universal testimony is, that the longevity of the poor is less, and generally much less than that of those whose circumstances supply them with the comforts of life. This last report states that the average longevity of paupers was 66, 70, and 71 years in the different years or different parts of the State. When put in comparison with the longevity of the other classes, this statement gives very wrong notions of the comparative worth of life in the various conditions or employments.

A large portion of the paupers are not old because they belong to that class, but they belong to this class because they are old. They sustain themselves through the active periods of life by their own exertions; and when, with advancing years their strength declines, they fall upon the public for support, and of course swell, in a very disproportionate degree, the average longevity of the class to which they belong at death. For many of these, pauperism is rather a condition than an occupation, and it might as well be designated old age as pauperism. A much more appropriate arrangement is to classify the paupers with the labourers.

Yet they have a questionable title to that class, inasmuch as so many of them belonged to other occupations, and were mechanics of every sort—farmers, seamen, and even merchants, &c., as long as their strength allowed them to attend to business or work. Strictly speaking, the paupers should be analyzed, and distributed among the various other classes from which they came to the poor-house; then we might see what employment exhausted them earlier or later, and deprived them of the power of self-sustenance, and what carried them on to old age when they could no longer work. For want of the means of making this analysis and distribution, we have omitted them in our classification.

The proper classification is not according to names, but according to sanitary influence. In a former article on this subject, we spoke at length of this, and gave an arrangement according to a law which seemed then to be suited to the nature and effect of the several employments on the health of those engaged in them. Time and further study have not changed our opinion in this matter; and we now repeat it, in order to explain the table of longevity in the occupations and classes of occupations.

Until we shall have very many more facts than we now possess in respect to mortality, we would condense all the occupations into ten classes:—

I. Farmers, gardeners, and all who cultivate the earth. They have constant, and sometimes laborious, exercise in the open air. But they have the means of living comfortably, and are free from anxiety.

II. Labourers, servants, brakemen, chimney-sweepers, coachmen, stagemen, hostlers, well-diggers, and workmen in powder mills. These men are gene-

rally subject to hard labour and much exposure. They live abroad. They are often deprived of some or many of the comforts of life.

III. Active mechanics employed in the open air. These are boat-builders, brickmakers, carpenters, caulkers, masons, millwrights, riggers, sawyers, ship-carpenters, stonecutters, and tanners.

IV. Those who are otherwise occupied actively abroad, but not in the cultivation of the earth: these are army-officers, baggage-masters, butchers, drovers, engineers and firemen, expressmen, ferrymen, lighthouse-keepers, news-carriers, pedlers, pilots, sextons, sheriffs and constables, soldiers, stablers, stevedores, surveyors, teamsters, watchmen, weighers, and gaugers.

V. Active mechanics, &c., working within doors. These are but little exposed to the elements; they work in warm rooms, and exercise constantly and sometimes severely: axe-grinders, bakers, basket-makers, blacksmiths, bleachers, bookbinders, brush-makers, cabinet-makers, calico-printers, card-makers, carriage-makers, chair-makers, chaise-makers, churn-makers, cloth-makers, clothiers, comb-makers, confectioners, cooks, coopers, coppersmiths, curriers, cutlers, distillers, dyers, file-cutters, founders, furnacemen, glass-blowers, glass-cutters, gunsmiths, harness-makers, hatters, leather-dressers, machinists, match-makers, morocco-dressers, nail-makers, painters, paper-makers, pianoforte-makers, pill-makers, potters, pump and block-makers, reed-makers, rope-makers, sail-makers, sash-makers, screw-cutters, soap-boilers, tallow-chandlers, tinsmiths, tobacconists, trunk-makers, turners, wagon-makers, weavers, wheelwrights, upholsterers, victuallers, and wool-sorters.

VI. Sedentary and quiet mechanics, &c., who are occupied, but without much exercise, in shops, &c., and protected from the elements. These are artists, barbers, cigar-makers, comedians, dentists, engravers, goldsmiths, jewellers, musicians, operatives, portrait-painters, printers, saddlers, shoemakers, shoecutters, silversmiths, tailors, watchmakers, whip-makers.

VII. Seamen and others, employed on the ocean and on the lakes, exposed to the severities of the weather, without protection, and deprived of many of the comforts which are usually enjoyed on the land. These are sailors, fishermen, marines, and naval officers.

VIII. Persons engaged in trade and finance; they are occupied in their houses, offices, or places of business. They have very little occasion for bodily exercise at home or abroad; but their brain is, in all, exercised to some extent; and, in fact, to a very great extent, with calculations, watching the variations of the market, and with the anxieties of business. These are merchants, traders, grocers, apothecaries, druggists, booksellers, stove-dealers, provision-dealers, clerks, manufacturers, speculators, bankers, bank-officers, brokers, capitalists, gentlemen, inn-keepers, postmasters, railroad agents and conductors, ticket-masters, and superintendents of factories.

IX. Persons whose occupations are almost exclusively of the brain, and who have but little or no occasion to labour with the body, in the discharge of their professional duties. These are clergymen, judges, justices, lawyers, physicians, professors, teachers, students, civil engineers, editors, and public officers.

X. Mechanics, whose occupations are not specified.

XI. Females.

There are many other things in these reports, to which we would gladly call the attention of the readers of the *Journal;* but here is all that we have space and time to present. We commend all of these documents to the public for the lessons of truth and wisdom they in various degrees teach; and we commend the example of those governments that have produced them, to all others who have not done like unto them. E. J.

BIBLIOGRAPHICAL NOTICES.

Art. X.—1. *Transactions of the Tennessee State Medial Society, at its Twenty-Fourth Annual Session, convened at Nashville May* 4, 1853. 8vo. pp. 79.

2. *The Transactions of the Iowa State Medical and Chirurgical Society, Third and Fourth Sessions, held at Fairfield May*, 1852, *and Davenport June*, 1853. 8vo. pp. 48.

3. *Transactions of the Medical Society of the State of New York, at its Annual Meeting, in the City of Albany, held February*, 1854. 8vo. pp. 148.

4. *The Minutes of the Fifth Annual Meeting of the Medical Society of the State of Georgia, held in the City of Macon April* 12, 1854. 8vo. pp. 55.

5. *The Transactions of the New Hampshire Medical Society, Sixty-fourth Anniversary, held at Concord June* 6*th and* 7*th*, 1854. 8vo. pp. 56.

6. *Proceedings of the American Pharmaceutical Association, at the Third Annual Meeting, held in Cincinnati July* 25 *and* 26, 1854. 8vo. pp. 40.

We have always regarded the published *Transactions* of the several State Medical Societies as among the most interesting portions of our medical literature; and we have, consequently, felt it our duty, as journalists, to give to them as early a notice as possible. Our shortcomings, in this respect, are to be attributed, not to any neglect on our part, but to the number of works in the different departments of medical science constantly claiming our attention, as well as to the irregularity with which the volumes of *Transactions* reach us.

The value of these *Transactions* is due partly to the contributions to medical knowledge, and to the ethics of our profession, furnished through the several addresses, reports, and other papers of which they are composed; but mainly, we conceive, from the evidence they furnish of organized and concentrated action among the physicians of the United States to elevate the standing of the profession and assert its rights.

In perusing the annual volumes of *Transactions* issued by the leading State Medical Societies, amid, it is true, some chaff, much valuable grain is invariably to be found. Were the reports and papers, communicated at the stated sessions of these societies, to be confined strictly to the personal observations and experience of the physicians located in the different sections of each State, in reference to the diseases of their respective localities, these *Transactions* would do much towards elucidating the etiology, pathology, and therapeutics of those maladies the American physician is called upon to treat. As it is, the facts recorded on their pages, and through them made the common property of the profession, have already had no inconsiderable influence in furthering the progress of medical truth. As the record of the proceedings of congresses of physicians, having for their object the educational, ethical, and social progress of the profession, these *Transactions* have especial claims to our notice.

"This country," says Dr. Kneeland, in an address delivered before the Onondaga County Medical Society, and contained in the *Transactions of the New York State Medical Society for* 1854; "this country has, more fully than any other, and this age, more than all past ages, come to be characterized as the home and the era of voluntary association for mutual benefit."

"Association in social, scientific, and fraternal conferences, teaches us and makes us *feel that we are brethren;* that our interests are common stock, so far as professional honour and standing are concerned; and that any means which lowers in the public mind our brother's character for honour and skill, takes so much capital from the joint-stock concern, and diminishes our own dividend of respectability.

"In meeting with each other, away from the petty rivalries and anxieties of our private practice, our opinions and motives are more likely to be seen and judged with kindness and impartiality; and we are brought to feel, on these occasions, that we are neither compelled 'to tread the wine-press' of our professional trials 'alone,' nor are we, if honoured and successful, 'alone in our glory.' And also, that being the oracle of a small circle of adoring, but undisciplined minds, is not *true and undisputed greatness*. That making use of our noble calling, as a mere money-making trade, is a vile desecration of a time-honoured and truly philanthropic vocation. On these occasions, we are brought to realize that our manhood, and the noble profession of our choice, demand higher aims and nobler aspirations.

"Our opinions and sentiments are here subjected to the test of kindly criticism, and, whatever of discovery or improvement we may suggest, will alike honour the author and benefit his auditors. Our errors and mistakes will more likely be corrected and remedied than if never tested in the crucible of scientific discussion.

"Knowledge is a species of wealth which is increased by imparting, and if our associations are made what their founders designed, and what their self-sacrificing sustainers hope them to become, every medical meeting will add to the practical knowledge and skill of all who attend, and we shall leave behind us some mistake, or crude, unformed opinion, and in its place bear with us to our fields of labour some fact or thought of practical value. Nothing quickens intellectual vigour more than earnest collision of mind with mind."

"By meeting our compeers we break the tether of selfishness, and escape from our daily, weekly, yearly round of drudgery and toil, wherein we are likely to form a hard-beaten track of routine practice, bare of food for the mind's growth. We enlarge our range of vision and mental nutriment, by coming in contact with minds which, having been subjected to somewhat different educational influences, and having looked at the same truths from another stand-point than we occupy, can teach us, even by differing from us, this, if no more, that knowing a thing is much short of knowing all about it, with its influences *upon* and varied relations *to* all other knowledge.

"Mark those physicians who enjoy but the intercourse of unprofessional minds, without finding their level by frequent contact with professional compeers; they stand aloof from medical associations, fearing they shall not receive due honour, with all that the admiration of partial friends and flatterers can do to puff and dilate them into overgrown self-importance, together with all that the ignorant and credulous may feel or fancy of their power."

"No class of men would be more benefited by frequent seasons of professional intercourse, away from the sick-room, away from the daily scrambles for power, and wealth, and fame; and, did they not fear the danger of losing their greatness, by getting the attenuated balloon of self-conceit ruptured by too rough a collision, we might oftener induce them to favour us with their august presence.

"Another, and by no means the least benefit to individuals, furnished by medical association is, that quackery, in all its protean forms, may be more effectually discovered and exposed by concerted action, than by every physician attempting to 'fight the common enemy on his own hook.'"

"Association may effectually aid us in elevating the standard of *professional morality*. By enforcing our discipline, and requiring obedience to our code of medical ethics, we may drive those demi-quacks who only use their connexion with us as a screen for debasing arts, and appliances to win public favour, forth to companionship with new-system empirics and shameless charlatans. And since *character*, and not numbers merely, is the chief means of our influence for good, let us guard the avenues to membership in our associations, so that no man, whose want of moral worth and character would unfit him for our personal esteem, shall be received among us as a brother; and as associational is but the totality of individual character, let us maintain, each man for himself, a high standard of professional integrity and honour; not the *mere* observance of etiquette, and obedience to codes and forms, which keep the selfish time-server within the statute from dread of penalties, but that lofty and universally

applicable rule which teaches us to *do to others* what they would have a right to expect *from us* in a change of relative circumstances."

"Association entered into with elevated views of its utility and importance, would tend to bind us firmly together, and to make us true to each other. Having decided upon the worthiness of a physician, and received him as a brother, we should be careful of his reputation. Having opened our arms to receive him, let us 'grapple him to our hearts with hooks of steel;' and if envy, malice, or detraction assail him, make common cause in his defence."

The foregoing extracts from Dr. Kneeland's address, express with great force and truth, in plain, familiar language, some of the leading advantages the medical profession derives from organization and association. We would add another, which the doctor has not noticed—the attainment of all those reforms, educational and ethical, which the wise and good within our ranks have so long laboured to achieve. We feel persuaded, that by a unanimity of action, which can be brought about only through an organization of the profession throughout the United States, legitimate medicine, practised by a body of well-instructed and skilful physicians, may be again made to exert its proper influence, so as to command the respect and confidence of the community at large. Though the schools may withhold from us their aid in the furtherance of this important object, though there be many in our own ranks who will do everything in their power to oppose it, having private interests to subserve, adverse to those of the mass of the profession, still, we only want unity of action among those resolutely bent upon attaining the desired end, to insure ultimate success.

1. The *Transactions of the Tennessee State Medical Society* contain a very sensible address by the President, Dr. John M. Watson, on the retrospective, perspective, and prospective views of medicine. It is true, we cannot entirely agree with the author in his anticipation of the period "when we shall have a separate branch of medicine for every organ"—"a name and a doctor for every part of the system, whose care and duty it shall be to treat the diseases of one particular organ in an open, scientific way," as one in which "we may expect all that can be achieved by man" in the preventing and cure of disease, "aided, as he will be, by strong collateral lights on every hand." We are not perfectly convinced that, by dividing and subdividing the practice of medicine into specialities, each having for its object the maladies of a particular organ, our acquaintance with therapeutics and general pathology will be much improved. The mutual relationship and influence of the morbid conditions of the several portions of the system must always be taken into consideration, in order to arrive at correct views in respect to the different maladies to which the human frame is liable—their pathology, their prevention, and their treatment—and this cannot be done if the diseases of each organ are studied and treated apart from those of every other organ.

The Address of Dr. Watson is followed by a "Report on the Adulteration of Medicines, Chemicals, Drugs, &c." The subject of this report is one of vital importance to the physician. To secure the purity and genuineness of the drugs and chemicals imported from abroad, is a wise protective measure, but is not of itself sufficient to eradicate the evil against which it was aimed, so long as there is no provision for restraining the adulteration and sophistication of drugs and chemicals subsequent to their importation; a practice carried on, we fear, to a much greater extent than is generally suspected.

The report of Dr. Currey closes by offering a few suggestions as to the best mode of remedying this evil, so far at least as the medical faculty is concerned.

Dr. Currey would have every physician so instructed as to be a judge of the genuineness and purity of the medicines he administers; not to be merely able to arrive at a *prima facie* judgment of their character from an acquaintance with their physical aspect, but to determine positively that they are pure and genuine by chemical analysis. He would have no one permitted to engage in the compounding and dispensing of medicines who has not qualified himself for the task by a thorough pharmaceutical as well as practical training. Thirdly, Dr. C. remarks, that foreign adulterated and deteriorated medicines having been declared contraband by Congress, it devolves upon our State legislatures to

devise the means by which home adulteration and sophistication may be arrested. More, however, in this respect, he believes, is to be gained by popular opinion, and by pharmaceutical and chemical education, than by legal enactments and penalties.

The history of a case of cerebral disease, one of false aneurism, and one of Fallopian pregnancy, and a description of a set of instruments, by which their inventor, Dr. J. L. Park, thinks "some of the inconveniences attending the operation for the radical cure of fistula in ano, whether by the knife or ligature, may be obviated," complete the professional papers embraced in these *Transactions*.

2. The *Transactions of the Iowa State Medical and Chirurgical Society* open with the annual address of the President, Professor D. L. McGugin, in which he presents a graphic sketch of the toils and anxieties, the sacrifices and exposure, the duties and responsibilities incident to the life of the physician; commencing from the period of his novitiate as a student of medicine to that of his death; for few members of our profession can hope to acquire the means to enable them to retire in season from its "exacting, sacrificing, and crushing" cares, and spend their latter days in quietude and peace.

The address of Dr. McGugin does not present only "the toil and conflict" of the physician's life, he also depicts his hours of triumph and rejoicing, his "seasons of enjoyment, if not of repose;" "his moments of happiness," although "not always seasons of personal comfort;" his fond hopes and Elysian dreams, though "not founded in the prospect of wealth, fame, or glory, but simply in the consciousness of having discharged his duty." Were it not for these bright spots strewn along his path, and rendered still brighter by their contrast with those of deep gloom he has daily to encounter, the life of the physician would present a tissue of unmitigated darkness, his feelings would become misanthropic, or he would abandon a profession fraught with all the elements of discomfort and discouragement. The inherent benefits of our science, and the capacity of its professors for the accomplishment of good, constitute its pillars and its props, and at the same time a source of proud satisfaction and consolation to those who devote themselves to its service.

A short address on medical education, by Dr. J. B. Elbert, delivered at the opening of the fourth session of the Society, held June 8, 1853, follows that of Professor McGugin.

The first of the *Reports* is devoted to the diseases of Des Moines, Louisa, and Washington Counties. It is very concise, and presents no prominent points of general interest.

The second report is on surgery, by Dr. John F. Sandford. It embraces the history of a number of interesting and instructive cases.

The third report is on obstetrics and the diseases of females. It is drawn up by Dr. D. L. McGugin, and comprises the details of four cases; the first of a separation of the symphysis pubis occurring during a rapid natural labour, followed by inflammation of the bladder, uterus, ovaries, and peritoneum. The case was managed by an energetic antiphlogistic treatment; by drawing off, by the catheter, the urine from the bladder as it accumulated; by the application of a roller, to prevent any movement of the injured parts by the action of the muscles of the pelvis and lower extremities; and by opiates, to procure rest and relieve the harassing cough. This treatment was followed by the slow, but steady, recovery of the patient.

The second case is one of excessive leucorrhœa, resulting from a deep fissure in the posterior lip of the os uteri; cured by the local application of a strong solution of nitrate of silver, followed by the *al. tinct. iodine* to the tumid lips of the os uteri and around the cervix; with chalybeates internally, porter, and a generous diet. The leucorrhœa was soon arrested, and the patient restored to a reasonable condition of health.

The third case is one of fissure of the nipple, cured by the application of a strong solution of nitrate of silver.

The fourth case is one of *post-partum* uterine hemorrhage, treated successfully by pressure upon the abdominal aorta.

Two short papers, the one giving an account of prolonged vital contractility in the heart of a fish, and the other of the *tænia solium* of the catfish, complete the contents of the volume.

3. The *Transactions of the Medical Society of the State of New York* open with the annual address of the President, Dr. Jenks S. Sprague. The subject of the address is medical education. The examination of the question, What manner of men ought those charged with the high trusts of practitioners of the healing art to be, in order to command the confidence and respect of an intelligent community? A considerable portion of this very excellent address is devoted to a consideration of the absolute necessity of practical and morbid anatomy to the physician and surgeon, and the duty of legislative bodies to afford every facility to the medical teacher and practitioner for procuring the material necessary to the prosecution of these important branches of professional inquiry, so far as this can be done without violating the noblest and most sacred sympathies of the surviving relatives and friends of the dead.

This address is replete with good sense and a manly defence of the rights and interests of a properly-educated and well-organized medical profession. The presence of the members of the New York Legislature at the delivery of the address, gives to the several positions assumed by the author, and the manner in which they are enforced, additional point and interest.

A case of extra-uterine conception is reported by Dr. W. H. H. Parkhurst, of Herkimer County. We see no reason for supposing this to be a case of extra-uterine conception; it resembles a number upon record, where it was evident that the fœtus, when near its full term, had escaped from the cavity of the uterus into that of the abdomen, through a rupture of the parietes of the former. The interest of these cases arises from the fact of the patient surviving the immediate effects of the accident, and the residence of the fœtus, for so many years subsequently, in the peritoneal cavity, without the occurrence of extensive and fatal inflammation.

A very able paper, on a subject "of great practical interest to the whole medical profession," the treatment, namely, of penetrating wounds of the abdomen, with punctured wound of the intestines, and of penetrating wounds of the trachea, by Dr. Alden March, is the paper next in order.

The principles of treatment of wounds of the abdomen, as laid down by Dr. M., vary according to the character of these wounds.

"1. Wounds in which the viscera protrude, but unattended with injury of the latter. "In cases of this description, the sooner the protruding part or parts are reduced, the better. If it be omentum, there is no great difficulty in accomplishing the object; but if an intestine has escaped at a small opening, and has been some time out, and is more or less distended with air or feces, the operation may be attended with some difficulty; and, rather than use harsh means, by too much handling and force, the wound should be moderately dilated, upon the same plan as that adopted in relieving the seat of stricture in an ordinary case of strangulated hernia.

"2. In penetrating wounds of the abdomen, with *protruded* and *wounded* intestine, the treatment will depend upon the extent of the wound in the intestine.

"*First.* If it be quite small, not larger than the end of a probe, the protruded intestine is to be gently reduced, without the application of ligature or suture to the puncture, and intrusted entirely to the recuperative power of nature for closure. "In such cases, the mucous or villous coat being long and loose, by moderate pressure is thrust into the puncture, so as to protrude at the opening in the peritoneal coat, and thus plug the orifice, where it is maintained by the gentle and equable pressure of the abdominal muscles, and the other portions of the abdominal viscera. "Nature's next step is to provide the means for, and to effect a permanent closure. This is accomplished by the exudation of plastic lymph from the exterior of the wound, by which this local point becomes fixed and adherent to the peritoneal surface with which it may chance to come in contact. This may be the peritoneal membrane lining the abdominal wall, or it may be the peritoneal membrane of another fold of intestine.

"*Second.* When the wound is considerably larger—say, of the size of a puncture made in venesection—the lips of it may be seized by the claw or 'mouse-tooth' forceps, deep enough to embrace the whole extent of the puncture, and by a fine silk ligature all that has been elevated by the forceps immediately below their extremities secured. "The extremities of the ligature should be cut off close to the knot, and the intestine cautiously and gently reduced. In this case, adhesive inflammation takes place before the ligature is cast off, so that the opening is permanently closed, while the ligature is said to find its way invariably into the alimentary canal, and pass off with the contents of the bowels."

"*Third.* In a class of wounds of the intestine, still larger, whether *longitudinal*, *transverse*, or *oblique*, the principles of treatment do not appear to be quite so well defined and settled. Some surgeons are averse to the use of any and all kinds of sutures, trusting entirely to the resources of nature; while others are advocates for sutures of various kinds, and variously applied, as the interrupted, the uninterrupted or continuous, and the Glover's suture. I shall not stop to describe the various modifications of these, or any sutures, but shall merely describe the one which appears most simple and philosophical, and recommended by some of the most modern authors.

"A small needle is to be armed with a single thread of silk or fine linen, having a knot fixed at the long end, and the extremity of the wound pierced with the needle from within to without, so that the knot may occupy the inner face or cavity of the gut; next, turn in the lips of the wound, so that the two peritoneal surfaces will come in contact with each other, and continue the Glover's suture, striking the needle through from within, so as to appear at least a quarter of an inch from the border of the wound on the peritoneal surface of the intestine. In this way, three, four, or half a dozen stitches may be applied, according to the extent of the wound; and at last cut the thread near the intestine. The two peritoneal surfaces adhere readily, and the ligature, as it ulcerates out, finds its way into the intestinal tube, and is removed with the feces.

"It has been advised by some surgeons, after securing the wound as above described, or by the overcast or whip-stitch, to bring the unexhausted portion of the suture out at the external wound, and there fix it, so that a greater amount of certainty may be secured in causing adhesion of the wound in the intestine to the wound in the abdominal wall. In this case, if the wound in the bowel should give way, it would not be likely to close till after its position had been fixed by adhesion, when the patient might recover with an artificial anus."

"The danger from *penetrating wounds of the larynx or trachea*," Dr. M. remarks, "may be immediate or remote.

"1. The immediate danger depends on hemorrhage in an excessive degree; as when the carotid and jugular have been wounded, or the thyroidal and laryngeal branches of the carotid.

"2. When the patient is in a half-insensible condition, a moderate flow of blood from the wound may find its way into the larynx, and accumulate in the air-passages of the lungs so as to induce asphyxia. The second, or more remote danger, is liable to occur in the course of three or four days after the infliction of the wound, at the period of the development of inflammation. The inflammation of the wound and the mucous membrane may be such as to cause a rapid and almost perfect occlusion of the air-passage, especially when the mucous secretion is increased in quantity and vitiated in quality, and becoming so tenacious that the feeble efforts of the patient are inadequate for its removal. Under such circumstances, the patient is in great danger of suffocation.

"The third, and still more remote danger, will depend on the occurrence of inflammatory action of a severe character, descending through the whole length of the trachea and its ramifications in the lungs, and terminating in a severe form of bronchitis.

"The indications of treatment are few, simple, and easily understood, and yet liable to be forgotten or disregarded in the excitement and hurry of the occasion. I believe, however, it is the fact that the young physician more often forgets that a *penetrating wound* of the windpipe is more dangerous and requires

a different mode of treatment from a simple incised wound in other parts of the body. He has been instructed to secure bleeding vessels, cleanse the wound, approximate its margins, and secure perfect apposition, by which union by the first intention may be effected.

"The first indication of treatment is the same in both cases, that is, to arrest the hemorrhage, if it has not already subsided. This is accomplished by ligatures to the divided arteries, and pressure upon the wounded veins.

"The mistake which the ignorant or careless practitioner is liable to fall into is, in closing the wound tightly, in the larynx and in the skin, with sutures." "If the chasm be at once drawn tightly together, the immediate risk is greatly enhanced, as already stated; and yet this is an error very frequently committed in the hurry of actual practice. There is another danger liable to occur: the air being likely to be infiltrated into the cellular tissue, during expiration, causing troublesome and dangerous emphysema."

The paper of Dr. March is followed by the history of a case of dislocated femur, by Dr. Samuel Shumay, of Essex County, New York. The reduction in this case was effected in the mode first taught, as Dr. S. informs us, by Dr. N. Smith, of Yale College, as early as the year 1815, and subsequently revived by Dr. Reid, of Rochester, in 1852.

The dislocation of the femur was backwards and upwards, on the dorsum ilii. Ether was administered, "so as to blunt sensibility without producing full anæsthesia." The leg was then flexed on the thigh, the thigh on the pelvis, diagonally to the opposite side, "thus using the femur as a lever, and the trochanter as a fulcrum, to throw the head of the femur on a level with the margin of the socket; and then, by a sweep outward and downward, throw the head of the bone in the socket."

The annual address of Dr. J. V. P. Quackenbush, before the Albany County Medical Society, follows next in order. The subject of this address is prolapsus of the bladder, as it occurs during labour, followed by biographical sketches of the late Doctors Lewis C. Beck and David Martin, of Albany. The account given by Dr. Q. of prolapsus of the bladder, as illustrated by three cases detailed by him, is highly interesting and instructive. The accident referred to, as the doctor remarks, is of very rare occurrence. In fact, it ought never to occur in any instance where the obstetrician is called in at the commencement of labour. Produced by "the pressure exerted by the descending head upon the fundus or middle portion of the bladder, at a time when it is partially distended with urine," it must be evident that the accident may be avoided in every case in which the obstetrician—as he should in the early stage of every labour—satisfies himself as to the empty condition of the bladder, as well as of the lower portion of the intestinal canal.

"The symptoms attendant on prolapsed bladder are very distressing; there is a painful sensation of fulness, tension, and pressure downwards in the situation of the pubes, with a feeling of dragging from the navel, or rather the midspace between the navel and pubic symphysis, constant desire to micturate, and inability to void urine on the exercise of the will, and sometimes an involuntary escape on each return of uterine contraction. On examination per vaginam, the finger will detect a soft, fluctuating tumour, filling up the anterior part and one or both sides of the pelvic cavity, below the fœtal head, and the patient will complain if steady pressure be made on it. The introduction of the catheter, which may be easily accomplished, will at the same time withdraw the fluid, cause the swelling to disappear, and relieve the characteristic suffering. Keeping in mind the possibility of the bladder being pressed downwards, as described, it is an essential duty never to puncture any fluctuating tumour in the pelvic cavity without being first assured that it is not vesical, by the removal of the urine by means of the catheter.

"A chronic prolapsus of the bladder is likely to follow its accidental descent in labour."

From the address of Dr. Kneeland, before the Onondaga County Medical Society, which follows that of Dr. Quackenbush, we have already, in the commencement of the present notice, made some copious extracts.

The next article is a very manly vindication of rational medicine, or the regular practice as it is termed, in an appeal to the public, by Dr. Merrit H. Cash, of Orange County, New York. The address is short, pithy, and much to the point. It presents a just picture of legitimate medicine, "in the hands of an educated, professionally trained, discriminating faculty." We fear, however, that the picture of Dr. Cash, well drawn and correctly coloured though it be, will not attract very generally the attention of that public for whose benefit it was executed; and although the production of one who, being "no longer a practitioner," has evidently "no personal interest to subserve or feeling to gratify in respect to medicine, other than devotion to the medical good of the community," but few of those who may be induced to look upon it will, from its correct representation of the claims to public confidence and support of "rational medicine and the regular faculty," be induced to eschew "nostrums, patent medicines, and the various phases of medical empiricism which assail popular credulity, and bind it captive to their triumphal car."

A biographical sketch of the late Dr. Harvey W. Doolittle, of Herkimer, and an abstract of the proceedings of the annual session, complete the contents of the volume before us.

4. The minutes of the fifth annual meeting of the *Medical Society of the State of Georgia* contain, besides the minutes, properly speaking, of the session, a catalogue of the officers and members, and the constitution and by-laws of the Society, only two reports of a professional character. The first of these is on the relative value of lithotrity and lithotomy, by Dr. L. A. Dugas. In this report, Dr. D. has very ably and clearly set forth the advantages which lithotrity possesses over lithotomy in cases equally favourable to either operation; secondly, the intrinsic advantages of lithotomy in cases equally adapted to either; thirdly, the disadvantages of lithotrity and lithotomy respectively; fourthly, the obstacles in the way of lithotrity; and fifthly, those which contraindicate lithotomy. The general conclusions of the reporter in reference to the relative value of the two operations will be learned from the concluding paragraph of the report:—

"Having," he remarks, "now presented, even at the risk of tedious repetitions, the arguments which might be urged for and against both lithotrity and lithotomy, in cases deemed equally adapted to either of these operations, and also indicated the circumstances that should be taken into consideration in determining upon the adoption of one or other operative procedure, it may be expected of us to be more definite, and to avow a preference for either lithotrity or lithotomy as a general plan of treatment. In deciding upon the propriety of surgical operations, or upon the merits of particular processes, it will be often found a wholesome rule, and one by which the solution may be readily obtained, to suppose ourselves in the condition of the patient, and then to ask what would be our election under such circumstances. We have very frequently, in this manner, determined at once the course to be adopted in cases previously involving much doubt and hesitation. With regard to lithotomy and lithotrity, we are free to acknowledge that, if we were affected with stone, and the circumstances were equally favourable to the success of either operation, we would not hesitate a moment in giving the preference to lithotrity. The advantages of lithotrity are numerous and important; whereas the only intrinsic or valid objections that can be alleged against it, under the circumstances referred to, are to be found in the facts that the operation may have to be repeated, and that it leaves the patient more liable to a recurrence of the disease than lithotomy. The advantages of lithotrity unquestionably very far outweigh all the objections to it. So thought those celebrated practitioners, Lisfranc, Dubois, Boisseau, &c., who did not hesitate to do homage to lithotrity and to the skill of Civiale, by submitting to be lithotritized by him."

The second report is on chloroform in surgical and obstetric practice. It is by J. A. Eve, M. D., and advocates in the strongest terms the use of chloroform, more especially to relieve the sufferings of parturition, in the more difficult and painful cases, in the prevention and subduction of puerperal convulsions, and in cases of instrumental labour.

We admit the propriety of the reporter's directions for the employment of

the remedy when the propriety of resorting to it has been decided upon. We cannot, however, agree with Dr. Eve in his general and unqualified eulogium upon the propriety and perfect safety of a resort to chloroform in surgical and obstetric practice. Is it strictly true, as asserted by the reporter, that "the safety of chloroform, in all departments of medicine, may be regarded as established, by the *almost perfect impunity* with which it has been of late years so extensively and *indiscriminately* used, or I might perhaps with more propriety say *abused*," that "it may be unhesitatingly affirmed that no remedy, of even half the power, has ever been so generally and *recklessly* employed with so few fatal results"?

Does the reporter himself agree with his own assertions, as given above, when he says that "we would desire most earnestly to impress on our younger brethren the *importance of the greatest care and circumspection in its administration*? Instead of concealing the fact that *fatal cases have occurred*, we would rather point to them as beacons to warn the unwary *of the danger that may result* from the *careless* and *indiscriminate use* of this *truly potent* and valuable agent." "I would not be understood as regarding chloroform, or any other anæsthetic agent, *entirely devoid of danger* in obstetric practice, or that it might be given *recklessly* and *with impunity* in all cases." "It should always be exhibited *cautiously*, and never when the patient is excited; her mind should be calm and composed before inhalation is begun, *otherwise its effects might prove very unpleasant, if not dangerous*." "I would not advise its indiscriminate use in all cases."

We regret that the reporter has not given us the evidence upon which the following statement is based: "In *many* cases of reported death from this agent (chloroform), which have been investigated, it has been found that chloroform had not been administered, or else that dissolution might be fairly attributable to some other cause."

5. The *Transactions of the New Hampshire Medical Society* contain but little of a strictly professional character—only one paper, in fact—an essay on "The Chemical Changes that take place in the Human Body while in a State of Disease," by William H. H. Mason. This is, unquestionably, a most important subject, demanding a thorough investigation on the part of those who have the time, opportunity, and talents for the task. In the essay referred to, Dr. Mason merely indicates the general bearing of the morbid chemistry of the living body in reference to the pathology and therapeutics of disease.

In the transition from the doctrines of absolute humoralism, which, at one period, reigned dominant in the medical schools, to those of exclusive solidism, which subsequently, and for a long period, held an equal sway, the morbid changes which take place in the fluids of the human body, and the part these changes play in the production of disease or of its phenomena were, in a great measure, if not entirely, lost sight of. It is only recently that the morbid chemistry of the blood and other fluids has begun to attract the attention its importance merits, and the true character of the abnormal states of these portions of the living body subjected to accurate observation. With Dr. Mason, we are fully convinced that "a great field is here opened for further research—that pathological chemistry will yet discover new and highly important changes in the blood, which will lead to new remedies and a more correct treatment in many diseases." We agree with him, that it is even more than probable "many deficiencies of the blood, if well understood, might be made up by a change in diet, or by medicine, calculated to supply such deficiency; and more particularly, when certain principles are in excess, neutralizing and counteracting agents could be applied with much more certainty of success." We fear, however, that even after we shall have become fully acquainted with the nature and extent of the chemical changes in the fluids of the body during a state of disease, unless we shall, at the same time, be so fortunate as to detect the true pathological conditions under which those changes occur, and the causes upon which they depend, the anticipation indulged in by Dr. Mason, "that every part of the science of medicine will be reduced to a mathematical certainty," will be very far from being realized.

A very admirable address, on "Conservatism in Medicine," was delivered

before the Society on the first day of meeting, by the President, Dr. Albert Smith, of Dartmouth College. The address is a well-written production, and inculcates sound principles, which, were they generally recognized and acted upon by the members of our profession, would do much to enhance its dignity and promote its usefulness.

The conservatism which Dr. Smith inculcates is not a stagnant acquiescence in existing doctrines and practices, a dormant indifference to all change and improvement, but a determined adherence to established truth and right, knowledge and experience, and a firm resistance to every innovation that bears not their manifest stamp. We fully concur with Dr. Smith, that "we need an element that shall have some restraining power over excitable and imaginative minds, that shall make them think and reason before they yield to delusion and folly. Even if conservatism grows so rigid as to occasion scepticism, it is far better for our art than too much credulity. It is this principle only on which we can rely for safety, amid all the dangers and extravagances which these inventive and progressive times impose; it is conservatism that must keep out errors—that must protect us from the follies of the visionary and the absurdities of the credulous."

"We need a conservative principle like this, to preserve the integrity and consistency of our profession. In an age like the present, an age of progress and improvement, unexampled in the history of the world, without some restraining or regulating force, there will be a tendency to excess and extravagance in both the theoretical and practical parts of our profession. It will not be empiricism alone we shall need to guard against—that will be too palpable and open to be mistaken—and will, of course, be shunned; but we shall need to be guarded against the new theories, the strange doctrines, the extravagant spirit of innovation and reform, that come not through science and experience; we shall need to turn a deaf ear and a cold shoulder to much that comes to us even under the sanction of the regular profession."

But, while Dr. Smith advocates a conservatism which shall guard the members of our profession from being carried away by every new theory, by false facts, one-sided observations, the vagaries of genius, or the brilliant dreams of the imaginative, with whom close investigation is folly, and analogical reasoning more convincing than the results of experience, he at the same time would warn them, on the other hand, against that spirit of conservatism which would wed them to that which is old, merely because it is old.

In the following remarks, made in reference to the recovery from disease, which occasionally occurs under the inefficient treatment of the homœopath, there is much truth:—

"The getting well, or not dying, is not all that is required of the healing art; there is such a thing as getting well 'right,' as justly remarked by an old, discriminating physician; that is, with all the inflammatory action subdued, with every vestige of the disease eradicated, and a restoration of all disturbed organic functions to a healthy condition. It is quite another thing to be left with organs struggling with half-subdued disease, and the kind, recuperative power of nature, striving to surmount it; only made tolerable, but, perhaps, never removed, by time, which, next to death, wears out all affections left lurking about the system."

The late Dr. Parrish was accustomed to remark, that, while regular scientific medicine is open to the admission of all truths, from whatever source derived, and is strengthened and perfected by every new accession of truth, all systems of empiricism, on the other hand, are adverse to every truth not originally embraced in the dogmas upon which they are founded; for, the moment other truth is received, they must necessarily fall to the ground. A somewhat similar idea is thus expressed by Dr. Smith, in the address under consideration:—

"Are we blind to truth, too conservative, because we are unwilling to acknowledge an indebtedness to any of the forms of empiricism, when we know to the contrary, and hold them all in the utmost detestation and loathing? The very few truths they contain do not save them from the most utter contempt. Why need we go to any sect or class to learn medical truth? It is ours the

world over. Any useful fact or discovery, by whomsoever made, belongs to us; and we have the undoubted right to say that, if there is any true system of medicine, we possess it. On what better basis could any profession be founded than ours—on antiquity, on science, and on experience?"

"There is no medical truth, or plan of medication under the sun, better than that we now employ, that we should not at once adopt, let who will make a hobby of it, or come from what source it may. All remedies, the world wide, are ours." "Unlike the systems of charlatanry, we are tied to no prescribed course or theory, but we are bound to know and to make use of *everything* that promises to be useful."

The reference made by Dr. Smith to the medical periodical literature of the day, is just and pertinent:—

"We know," he remarks, "how essential this is to the progress of our art, and how much it aids in bringing out and recording all the important facts and hints that occur, ready at hand, when a sufficient number have accumulated, to be used in developing medical truth. It is a means by which we may also record the results of our own experience, and perfect ourselves in arguing and expressing our thoughts in written language. It keeps us advised of all the important passing events in our sphere; heralds forth fresh discoveries, new modes of treatment, new uses of old articles, new phenomena, and strange appearances. We learn of the advent of new books; we are advised of their value, whether they will repay careful perusal. They stand the worthy exponents and representatives of our body, ready to vindicate us, and do battle, if necessary, among all the hostile legions of empirics and 'irregulars' of every grade. By their ability, dignity, and bearing, they give character and power to the profession; they infuse a right spirit into our body—a spirit of inquiry and observation, of perseverance and enterprise, that are of incalculable value. It is the life's blood of the profession, and none but a low and unworthy conservatism could urge a word against such important auxiliaries. Our medical periodical literature must be sustained, and it can be without much burdening the profession. It cannot be said they are too numerous. They have thus far only kept pace with the demand, and hardly equal the periodical literature of other great interests in society.

"At the present time, no physician, who has ambition to claim a respectable standing, can really attain it, unless he is careful to be thoroughly versed in, and familiar with, everything relating to his vocation. How shall he do this? No man can do it without effort; he must read and think, and, while striving to learn much unknown before, also to *unlearn* much that has hitherto constituted his medical capital, which, he is now satisfied, is wrong and unfounded. The medical journals must be carefully read, new books must be procured and consulted, and every discovery and improvement thoroughly investigated. We should have a horror of falling behind the age. The physician will find no ease; his scanty leisure from business engagements will leave little time for study—none for indolence, for gossiping, or lounging about the town or village; and does he wish to be a *full* man, he will industriously gather up these fragments of time, and, amid fatigue, embarrassments, and interruptions, yet acquire a large amount of knowledge."

The address just noticed is followed by an oration by Dr. Andrew McFarland, on "The Poetry of the Medical Profession." It is a well-written, pleasing production; disfigured, it is true, by an occasional conceit, and not always happy in its delineation of the true poetry of the life, and duties, and pursuits of the physician; the lights and shadows, the trials and triumphs, which checker with pain and pleasure the career of the most successful.

Appended to these *Transactions* are: 1st. A graphic account, given by Dr. Nathan Sandborn, of his escape from the Norwalk disaster, May 6, 1853; 2d. A short biographical notice of Dr. Josiah Bartlett, who was among the victims on that occasion; and 3d. Extracts from a Lecture on the Rights and Liabilities of the Physician and Surgeon, by Joel Parker, LL. D., Professor of Medical Jurisprudence in the Medical School of Dartmouth College, in the Boylston Medical School, at Boston, and in the New York Medical College. These extracts have been already extensively circulated in our medical journals.

6. The last of the publications, the title of which is placed at the head of the present notice, is the *Proceedings of the American Pharmaceutical Association*, at its annual meeting of 1854. Upon the proceedings of this Association, from its first organization in 1851, we have looked with the deepest interest. "The science of pharmacy should ever be regarded as the handmaid to the science of medicine;" it must at least be evident that whatever is calculated to elevate and improve the condition of the first will react favourably upon the interests of the latter. "The pharmaceutist stands between the physician and his patient. As the compounder of his prescriptions, the physician's success is, to a certain degree, in his hands. Such a responsible position requires a thorough pharmaceutical as well as practical training;" and we look with great confidence to the American Pharmaceutical Association as an efficient agent to elevate the profession to its proper position, by requiring of its members higher qualifications, both moral and educational, than merely the ability to perform the mechanical and financial business of a drug establishment. We look also to the action of this Association for the establishment of means calculated to insure the purity and genuineness of the drugs and medicines used and dispensed in the various pharmaceutic establishments of the United States.

The several papers which form the appendix to these proceedings will be read with interest by both physician and apothecary. D. F. C.

ART. XI.—*Reports of American Institutions for the Insane.*

1. *Of the Maine Insane Hospital, at Augusta*, for 1853.
2. *Of the Massachusetts Lunatic Hospital, at Worcester*, for 1853.
3. *Of the New York Insane Asylum, at Utica*, for 1853.
4. *Of the Bloomingdale Asylum, New York City*, for 1853.
5. *Of the New Jersey Lunatic Hospital, at Trenton*, for 1853.
6. *Of the Pennsylvania Hospital for the Insane, near Philadelphia*, for 1853.
7. *Of the Pennsylvania State Hospital, at Harrisburg*, for 1853.
8. *Of the Western Asylum of Virginia, at Staunton*, for 1853.
9. *Of the Ohio State Asylum, at Columbus*, for 1852 and 1853.

1. AT the *Maine Insane Hospital*, the number of

	Men.	Women.	Total.
Patients at the beginning of the year was . .	50	34	84
Admitted in course of the year	65	59	124
Whole number	115	93	208
Discharged, including deaths	54	35	89
Remaining at the end of the year . . .	61	58	119
Of those discharged, there were cured . .	28	17	45
Died	11	4	15

Deaths from apoplexy 4, consumption 3, general paralysis 6, pneumonia 1, and laryngitis 1.

No epidemic prevailed, and no suicide occurred during the year.

We commend to the physicians of other asylums the method pursued by Dr. Harlow, in reporting the complete list of patients. The following is a synopsis of it.

		Disch'd cured.	Improved.	Unimp'd.	Died.
Persons admitted	1033	404	190	234	118
" re-admitted	194	79	43	33	18
" admitted a 3d time	50	26	2	7	2
" " 4th "	17	10	1	3	2
" " 5th "	11	8	2	0	0
" " 6th "	5	4	1	0	0
" " 7th "	5	2	2	0	0
" " 8th "	2	1	0	0	1
" " 9th "	1	0	0	1	0
Cases aggregate	1318	534	241	278	141

Of these, 71 were homicidal, and 129 suicidal; yet no accident has ever occurred from either of the former, and but two of the latter have destroyed themselves at the asylum. One of the homicidal men, removed from the institution against the advice of its officers, killed a man with an axe, in September, 1853.

Another wing for female patients is to be added to the establishment.

2. The *Massachusetts State Lunatic Hospital* went into operation in 1833. Under the energetic executive guidance of the late Dr. S. B. Woodward, it soon became very extensively known; and it has generally been esteemed, even to a comparatively recent period, not only as one of the best conducted, but also as one of the best constructed establishments of the kind in the country. But its Trustees now assert, in their report prefixed to that of Dr. Chandler, that it "has not only ceased to be regarded as a model institution, but it has fallen into the rear rank in the march of improvement." The halls are "low-studded, being only eight and a half, and nine feet high. They are warmed by furnaces which are very dangerous, and now nearly worn out. Their ventilation is so imperfect as not to deserve the name. The frequent occurrence of erysipelas is but one of the indices" of these defects. "There are forty-eight strong rooms, or cells, nearly all of them constructed of solid masonry, with iron doors." Some of them "are totally unfit for human habitations. There is an entire want of suitable yards (airing courts) connected with the buildings."

For these reasons, the Trustees, after having visited twenty-six public institutions, ten of them for the insane, in several of the States, recommend the construction of another establishment, and the evacuation and sale of that which now exists.

Let us follow these gentlemen in their tour of observation. "The Superintendents of the Lunatic Hospitals laid us under particular obligations for their generous courtesy, and the very frank and unreserved manner in which they exhibited their establishments, together with their methods of management and modes of treatment, pointing out improvements and criticizing defects. * * * No cell was found without a wooden floor, a wooden door, and plastered or ceiled walls. There is not a brick and mortar cell, with iron doors in either of the public establishments of the great States of New York and Pennsylvania, nor in the New Jersey State Lunatic Hospital, the design of which is quite generally regarded as a model.

"In the large establishment on Blackwell's Island, containing at the time of our visit, 574 patients, but two were found locked up, and those only temporarily, for a part of the day. The Superintendent, Dr. Rawney, stated that half-a-dozen strong rooms would be sufficient for that establishment, as not more than five or six a day were ever locked up, and those only for a few hours at a time. These rooms were used as sleeping apartments, and did not differ materially, in size, form, and appearance, from the other rooms occupied by patients, except the doors, which were stronger. These remarks in reference to the strong rooms are applicable to all the other establishments visited by us. In nine hospitals out of New England, containing about 2,250 patients, the number found under restraint, by being locked up, was only six. In one instance only had that restraint been continued more than two days, and that one had not exceeded a week.

"In the State Hospital, at Utica, but one person was found under restraint, and that one was confined in what they called a chicken-coop bedstead. There, they were in the process of demolishing their strong rooms, as useless. Their hospital was built some ten years after ours, and their strong rooms were comfortable apartments compared with ours, having wooden floors and plank doors, and were each furnished with a bedstead and bed.

"There are no more perfectly warmed and ventilated establishments in the country than the New York Hospital, the Pennsylvania Hospital, the New Jersey State Lunatic Hospital, the State Lunatic Hospital at Harrisburg, and the State Lunatic Hospital at Utica. In all these establishments, steam is the agent employed, and it gives perfect satisfaction. The plan at Utica, being

the most recent, is believed to combine more of modern improvements than any other. The Hospital at Utica, in all its appointments, may justly be regarded as a model institution, although its ground plan, in our opinion, is inferior to that of Trenton and Harrisburg."

That the buildings of the Massachusetts Hospital are very defective as compared with those of the institutions of recent origin, there can be no doubt; but it is very evident, that the Trustees have placed those defects in as bold relief as possible, by exhibiting them in the light of the broadest contrasts.

In reference to the practical application of the doctrines of Pinel, the Trustees make the following remark:—

"*Dr. Tuke, being the Superintendent, and possessing the whole control, found but little difficulty in testing them in practice at the York Retreat.*" No "Dr. Tuke," and no man named Tuke, was ever Superintendent of that Institution. We never heard, in England, of *any* Doctor Tuke. This is not the first time, however, that a *myth* of that name has been introduced into the profession, by persons on this side of the Atlantic. Samuel Tuke, for many years one of the Trustees of the Retreat, and well known by his publications upon insanity, was a tea-merchant, in York. Of the occupation of his ancestor, who was a member of the first Board of Trustees, we know nothing.

We now come to the report of Dr. Chandler.

	Men.	Women.	Total.
Patients in the Hospital, Dec. 1, 1852 . .	264	268	532
Admitted in the course of the fiscal year . .	136	152	288
Whole number	400	420	820
Discharged, including deaths	134	166	300
Remaining Novembr 30, 1853	266	254	520
Of those discharged, there were cured . .	65	80	145
Died	21	20	41

No patients were received excepting such as were sent by order of the Courts, or by the Overseers of the Poor of towns. The Hospital was intended for a number of patients not exceeding 400; yet, at one time in the course of the year, there were 567. Of the 520 at the close of the year, 216 were foreigners; that is, not natives of Massachusetts; and of the latter number, 140 were Irish. "The Irish," says the report, "are almost invariably State paupers. Only three instances have come to my knowledge of their bills, or any part of their bills, having been paid by themselves, or by their friends."

	Men.	Women.	Total.
Whole number of patients from 1833 to 1853 inclusive	2239	2225	4464
Discharged, recovered	994	1059	2053
Died	253	239	492*

Causes of Death.—"Marasmus 78, consumption 58, apoplexy and palsy 53, maniacal exhaustion 49, epilepsy 45, disease of heart 20, disease of brain 20, suicide 19, lung fever 18, diarrhœa 18, erysipelas 15, old age 13, typhus fever 11, dysenteric fever 9, inflammation of the bowels 8, dropsy 8, hemorrhage 6, gastric fever 5, cholera 4, cholera morbus 4, chronic dysentery 4, mortification of the limbs 3, disease of the brain from intemperance 3, bronchitis 3, hydrothorax 3, congestive fever 2, convulsions 2, land-scurvy 1, concussion of brain 1, disease of the bladder 1, fright 1, rupture 1, asthma 1, cancer 1, pleurisy 1, jaundice 1, chorea 1."

Dr. Chandler gives a table, in which 406 of the patients who died are included, showing the relationship of insanity to longevity. Its substance is as follows:—

* In one table the report gives but 491. The number of admissions in 1835, is stated in one place as 113, in another, 119. This discrepancy affects the whole number, making it but 4458, if the 113 be correct. We have quoted from the table in which the number of each sex is given.

	Years.	Months.	Days.
Average age of 201 males when attacked	42	8	10
Average age of 205 females when attacked	39	1	10
Average age of the whole number, 406	40	11	1
Average duration of insanity before admission, 201 males	4	2	9
Average duration of life after admission, 201 males	1	9	24
Average duration of life after the attack, 201 males	6	0	3
Average duration of insanity before admission, 205 females	3	3	6
Average duration of life after admission, 205 females	1	7	29
Average duration of life after the attack, 205 females	4	11	5
Average duration of life after attack of the whole number, 406	5	5	20
Average age at death, 201 males	48	8	13
Average age at death, 205 females	44	0	15

"The chance of life," says the report, "for persons in health at corresponding periods, as calculated and acted upon by life-insurance companies, is four times greater than is here exhibited for the male, and more than five times greater for the female. This shows, pretty conclusively, that insanity, when not recovered from, tends to shorten life."

3. The movement of patients at the *New York State Asylum*, in course of the fiscal year ending November 30, 1853, was as follows:—

	Men.	Women.	Total.
Patients at the commencement	215	210	425
Admitted	251	173	424
Whole number	466	383	849
Discharged, including deaths	227	176	403
Remaining at end of the year	239	207	446
Of those discharged, there were recovered	95	74	169
Died	19	20	39

Deaths from phthisis, 11; exhaustion, 7; general paralysis, 5; epilepsy, 3; exhaustive mania, 2; apoplexy, cerebral effusion, tumour of brain, pericarditis, hypertrophy and dilatation of heart, typhoid fever, dysentery, chronic diarrhœa, phlegmonous erysipelas, erysipelas from wounds received prior to admission, contusions received before admission, 1 each.

"The seven cases reported as having died of exhaustion, were wasted by disease or vicious habits. Three of them, at the development of mania, had been purged, blistered, and profusely bled, and were brought to the Asylum on beds. This injudicious treatment cannot be too strongly condemned. The recuperative powers in these were so far exhausted that no amount of care, stimulation, and nutrition could arouse them. It may be proper to state that a number of persons received in a state of extreme feebleness, after long nursing, watchfulness, and free stimulation, recovered."

"No case of suicide has occurred for more than two years."

The number of men admitted was greater than in any preceding year; that of women was diminished by an inability to receive them, on account of the extensive alterations in their department, necessary for the introduction of the new apparatus for heating and forced ventilation. Sixty applications were rejected, and forty-seven incurable cases discharged, to make room for curables.

In eleven of the cases admitted, the insanity was complicated with epilepsy. "In six of them, epilepsy came on at puberty, preceded the derangement, and was the exciting cause. In the others, the epilepsy commenced in childhood, and early induced dementia. In the table of causes, two of these cases are put down to the intemperance of the father; the father being represented as in a state of beastly drunkenness most of the time for a few years previous to their birth. Nine of the whole number of epileptics had a drunken parentage, and, in some, this vice extended several generations back.

Of seven cases in which the mental disorder was accompanied by general paralysis, six were of intemperate parentage, and three of the six "had a drunken and licentious ancestry." In fourteen patients, ten men and four women, the insanity is ascribed to "spiritual rappings."

"No epidemic prevailed during the year. One case of variolous disease appeared in May, which was immediately isolated, and the entire population of the house vaccinated, after which no other case occurred. This was a case of acute dementia, of eight months' standing and seven months' residence. *The mental affection entirely disappeared simultaneously with the full eruption of the disease.*"

There may be imprudent haste, as well as unwise delay, in regard to the removal of insane persons to the institutions devoted to their treatment. Especially is this true when the removal involves a long and wearisome journey, which the patient, disabled by physical disease or debility, is unqualified to bear. A woman mentioned in this report, while labouring under acute puerperal mania, "was brought from a distant State, *a journey of eight hundred miles*, THREE WEEKS AFTER DELIVERY, and was wasted to a skeleton, and not able to speak when received. She had not taken food or drink for four days, nor slept for seventy-two hours." If any reader should have a doubt as to the result of this case, he may remove that doubt by referring to the report.

In the record of general results already quoted, it will be perceived that 169 cases are reported as "recovered." In a subsequent and more specific table, these recoveries are arranged under two heads, viz: "Well," and "in usual health." Of the former there are 126, and of the latter 42. Our former notices of the reports from Utica have given Dr. Benedict's reasons for this distinction. A similar method of reporting is adopted in some of the German institutions. It is, probably, a more accurate method than that which is the most generally in vogue.

As the results of the industry of the female patients and attendants, it is stated that they repaired all the clothing and bedding, and made 5,760 garments and articles of domestic use. The tailors' shop produced 60 coats, 149 vests, and 205 pantaloons; and the carpenter shops, numerous articles of household furniture.

Aggregate of patients, from Jan. 16, 1843, to Dec. 1, 1853 .	3,923
Discharged recovered	1,625
Died	446

4. From the general statistics of the *Bloomingdale Asylum*, we abstract the following:—

	Men.	Women.	Total.
Number of patients, Jan. 1, 1853 . . .	52	67	119
Admitted in course of the year	73	62	135
Whole number " "	125	129	254
Discharged, including deaths	69	61	130
Remaining, Dec. 31	56	68	124
Of those discharged, there were cured . .	21	28	49
Died	13	9	22

Deaths from typho-mania, 4; chronic mania, 4; abscess in the brain, 3; epilepsy, 3; *paralysis générale*, 3; puerperal mania, 2; mania-à-potu, 1; apoplexy, 1; serous effusion within the cranium, 1; pleurisy, 1.

Seven of the patients died within one week after admission.

The subjoined extract presents the views of Dr. Brown in regard to the numerical method as applied to insanity:—

"The terms *recovered*, *improved*, and *not improved*, as used in reports of this character, must necessarily be in some degree indefinite in their signification; they represent only the *opinion of the reporter* on the cases embraced in the opposite numerals. That this opinion will be determined, or modified by the observer's temperament, is a fact so well known to those familiar with the sub-

ject that the 'statistics of insanity' are very generally regarded as collections of individual opinions, rather than as reliable scientific data. With the sincerest desire to arrive at entire accuracy, it is not unfrequently difficult to determine the exact state of the mind at the moment of the patient's discharge. While, in one instance we may be discomfited by the sudden relapse and return of one dismissed as convalescent, our chagrin may be soothed by the assurance that another, whose removal we had strongly resisted as imprudent and critical, has progressed to complete restoration. It may even be somewhat questionable whether that degree of improvement which justifies enrolment among the 'recovered,' can, in every case, be adequately determined as the patient is leaving the asylum, as yet unsubjected to the test of association with the world, and unexposed to influences which may have produced his disease.

"The marked contrariety of opinion as to the justice of characterizing certain phases of a still existing malady as an *improvement*, may well qualify confidence in the numerical method of estimating results of treatment in mental diseases. The subsidence of agitation, noisy declamation, and violence, followed by a prolonged period of calm, does not necessarily indicate a better condition of mind; nor do improved physical health, and discontinuance of bad habits, invariably point toward recovery. Yet, each of these supposed changes is desirable as an improvement on its antecedent state, and, while some physicians *exclude* from the class of *improved* all cases in which an approach towards recovery from the mental derangement be not apparent, others, with equal respect for truth, *admit* all in which the above-named desiderata are attained."

So fully have we concurred in the opinions advanced in the paragraph last quoted, that, for several years, in making our extracts from the statistics of the reports, we have entirely omitted those under the heads, "much improved," and "improved." That the temperaments of the superintending physicians of the various asylums differ, it is reasonable to suppose; and that the judgment of each physician is somewhat influenced by his specific temperament, is a proposition which will not be contested by any person much versed in physiology and psychology. But, that this influence is sufficient to destroy our confidence in the statistics of *cures*, reported agreeably to a sincere conviction of truth, we cannot believe. If it be, the sooner the practice of reporting them is discontinued the better will it be for the progress of true science.

There is much truth, as well as appropriateness, in the following remarks near the close of the report before us:—

"We have been too prone to regard the *balance sheet*, the *farm account*, and the *report of articles manufactured* as matter of special solicitude, contemplating the patient as an agent in the industrial hive, rather than as the object of all the accumulated means of treatment. In the lunatic hospital, as in society and in the State, the individual must be prominent. The very disease for which he is admitted tends ultimately to destroy individuality. For this reason his identity must be preserved, his just claims recognized, his self-respect encouraged, and his mind incited to useful or refining occupation. In this kind of moral treatment, some of our co-labourers of the Old World excel us. To emulate their merit, we need a courageous zeal which shrinks from no obstacle, a generous enthusiasm that waits not to weigh restored minds against a diminished credit balance, and the stimulating conviction that laurels yet ungathered line the steeps above us."

Large additions to the "lodges" of the Asylum have recently been erected, and the new method of heating, in connection with a forced ventilation, introduced into those buildings. The number of applicants for admission into the institution is greater than the means of accommodation. In 1836, there were upwards of 160 patients. The departments now occupied by patients are at least fifty per cent. more extensive than at that time; yet Dr. Brown proposes to limit the number, in future, to 150. This recognition of the importance of sufficient room is one among many evidences of improvement.

	Men.	Women.	Total.
5. By the report of Dr. Buttolph, it appears that the number of patients in the *Asylum at Trenton*, Jan. 1, 1853, was	91	91	182
Admitted in course of the year	56	63	119
Whole number	147	154	301
Discharged, including deaths	49	47	96
Remaining January 1, 1855	98	107	205
Of those discharged, there were cured	27	26	53
Died	10	7	17

Deaths from general exhaustion 4, consumption 3, epilepsy 3, apoplexy 4, congestion of brain 1, congestion of lungs 1, chronic diarrhœa 1.

The patients enjoyed "a remarkable exemption from all acute and epidemic diseases" throughout the year.

The liberal donation from Mr. Randolph, mentioned in the last preceding report, has been devoted to the construction of an octagonal stone building, thirty-two feet in diameter, lighted from the top, and surrounded by a portico eight feet in width. The interior will be finished in a style appropriate for a handsome reading-room and museum.

	Men.	Women.	Total.
Patients admitted from May 15, 1848, to Dec. 31, 1853	320	314	634
Discharged, recovered	108	106	214
Died	36	35	71

To this brief report are appended the "Propositions relative to the construction of Hospitals," and those "On the organization of Hospitals," which have been issued by the Association of Medical Superintendents of American Institutions for the Insane. Had that Association achieved no other good, the production of these two documents would alone have been a sufficient recompense for all its labours. In future, should there be a hospital for the insane erected, and put in operation, with the imperfections of those which were established twenty years ago, it will not be for the want of available means for their prevention.

6. The report of Dr. Kirkbride, for 1853, furnishes the following statistics of the movement of the inmates of the *Pennsylvania Hospital for the Insane* in the course of the year:—

	Men.	Women.	Total.
Patients December 31, 1852			215
Admitted since that time			191
Whole number	205	201	406
Discharged, including deaths	93	78	171
Remaining Dec. 31, 1853	112	123	235
Of those discharged there were cured			88
Died	10	5	15

Deaths from acute mania 4, softening of the brain 3, exhaustion from long-continued refusal of food 2, tubercular consumption, chronic inflammation of the lungs, chronic diarrhœa, disease of the bladder, sloughing of the perineum, and old age, 1 each.

Of seven patients prematurely removed from the hospital, five were believed to be curable.

	Men.	Women.	Total.
Patients admitted since the opening of the hospital	1299	1099	2398
Single	708	427	1135
Married	536	530	1066
Widowed	55	142	197
Cured	622	515	1137
Died	142	103	245

Insanity commenced before the patient was 10 years of age, in 5; between 10 and 20 years, in 282; 20 and 30 years, in 915; 30 and 40, in 577; 40 and 50, in 290; after the fiftieth year, in 229.

During the whole of the past year, "the institution has been rather more than comfortably filled, the average number being 229, while 220 is regarded as the capacity of the building." The highest number was 248. Some applications for admission were refused. The elaborate system of moral management heretofore pursued at this hospital, and pretty fully described in our previous notices of the reports emanating from it, is still continued. We still await, however, the introduction here, as well as at all the other similar establishments in the United States, of one feature in the general treatment, without which it is believed that no institution for the insane can be perfect. We allude to an active, thorough, energetic system of disciplinary, gymnastic, hygienic, physical and mental improvative and curative management of the chronic cases—even of those who may have been more or less demented, torpid and stupid, and perhaps given up as incurable for years. In short, a school for idiots, technically speaking, is needed in every large institution for the insane. We have wonderful results from those schools in Germany, Switzerland, France, England, and, to some extent, in this country, where the subjects were congenitally imbecile. We anticipate success no less eminent among those whose demention is acquired, for we are fully convinced that the physical lesion to be overcome is, in a large proportion of cases, a less discouraging obstacle in the latter than in the former. All things, and especially the *power*, by his facilities for the acquisition of the means, point out Dr. Kirkbride as the man to become the pioneer in this undertaking. The hospital under his superintendence already approximates so nearly to perfection, that there is some danger of his becoming the Alexander of his sphere, and weeping that there are no more realms to conquer. But while among his patients, one imbruted remnant of that which was once a man, moves only in obedience to the calls of nature, and of his attendant, perhaps to the latter alone, everything is not accomplished. While along the benches, or on the floors, in corners or partially secluded nooks, lying, sitting, crouching, or standing in listless inactivity, are those who still bear some relic, how slight soever it may be, of their former intellectual manhood, so long will the necessity be indicated for that systematic, physical, and mental schooling, which has been mentioned. Be it understood, that we are pointing to an entirely new era in the history of our institutions, specially devoted to the insane; an era within the first gleams of the aurora of which we have been brought by the progress of the last half century. We believe, that at the present time, the class of patients in question receive no more efficient treatment in any of our Asylums, than in the Pennsylvania Hospital; and we have alluded to them there by partial description, only because we know that such a class exists, and, but too often, a very numerous class, in every establishment of the kind.

Among the important improvements of the past year, mentioned in the report before us, is a serpentine carriage-road through the pleasure-grounds of the department for females. A similar road is in progress through the grounds devoted to the men. When the latter is completed, a drive of one mile and three-quarters can be taken within the walled inclosure of the Hospital.

Gas has been introduced, for lighting the buildings, and with three times the amount of light formerly furnished by oil, the actual expense is less.

Dr. Kirkbride devotes several pages to the discussion of the question whether insanity be increasing in a greater ratio than the population. "It is not difficult," he remarks, "to understand that there may be elements in operation in this country more likely to produce mental derangement than in most others; but at the same time, it must also be conceded that other causes, elsewhere prevalent, are here absent; and which, different as they are in their general character, tend to produce nearly the same effects. While the general prevalence of comfort among our whole population, the comparative ease with which nearly every individual may earn a livelihood, and the absence of tyranny and a grinding oppression of the poor and dependent, ought to contribute no less to the mental than to the physical well-being of the whole community; still,

some of the characteristic traits of our people, originating in this happy state of things, tend to a different result. The very active and wide-spread commercial speculations of our citizens, the incessant taxing of the mental and physical powers to their utmost, the absorbing pursuit of business, aiming at rapid success, and the hasty accumulation of wealth, is a state of constant mental anxiety, of labour without relaxation; and it is too often a mere lottery, in which great and sudden good fortune is the exception, and loss and disappointment the more common, though less noted results." The man of business, when able to retire, is unfitted for the change; "he discovers, with surprise, that long habit has rendered the excitement, the toil, and the anxieties of business, great as they may have been, among the necessaries of his existence; and irksome as he may occasionally have found them, he now concludes that they are infinitely preferable to the *ennui* which presses so heavily upon him. Fortunate is the man thus situated, who can take a hearty, permanent interest in other pursuits, who can engage in works of benevolence or of public utility that will render him not only a benefactor to his species, but will also preserve him from an indulgence in habits that may ruin him physically, and from yielding to feelings which may seriously impair the functions of the mind." After mentioning other causes, the conclusion is arrived at, that, "it will probably be found that the number of cases (of insanity) among us has *not* increased in a greater ratio than that of the general population." The greater prominence, during the last few years of the subject of insanity, and of *its* subjects is mentioned as only an apparent, not a real indication of the increase of the disease. The filling up of the hospitals, also, "does not prove that insanity increases more rapidly than the population." "Philadelphia, in 1830, had accommodations for 385 insane, with a population of 188,961. At the end of 1840, with a population of 258,037, she could provide for about 530, and now, with half a million of inhabitants, her different institutions can receive 630 patients." Thus, in regard to that city, the provisions for the cure of the insane have not kept pace with the population.

But further accommodations are needed, and hence Dr. Kirkbride suggests, "that a new Hospital, replete with every modern discovery, and all the improvements suggested by a large experience, and capable of accommodating 200 male patients, should be erected on the seventy acres of land now comprising the farm of this institution, and directly west of its present inclosed pleasure-grounds; while the present buildings, with everything included within our external wall, should be given up for the exclusive use of a similar number of females."

7. We glean from the report of Dr. Curwen, the subjoined sketch of the movement of the population of the *Pennsylvania State Hospital*, in 1853.

	Men.	Women.	Total.
Patients in the Hospital Dec. 31, 1852 . .	59	47	106
Admitted in course of the year	95	68	163
Whole number	154	115	269
Discharged, including deaths	55	32	87
Remaining, December 31, 1853 . . .	99	83	182
Of those discharged, there were cured . .			27
Died			17

Causes of Death.—Epilepsy 5, exhaustion consequent to chronic mania 5, paralysis 3, "disease of the lungs" 2, acute inflammation of the brain 1, gradual decay of the vital powers 1.

"The general health of the household has been good. We have been spared the visitation of any epidemic, and only a few cases of disease incident to the season were under treatment during the summer and autumn." "Several of those who were much improved at the time of their removal, subsequently regained their former mental vigour." "A little girl, three years and four months old, evincing unequivocal symptoms of mental disorder, was admitted in the early part of the year. The mental disorder was recent. This case, so

interesting on account of the age and mental peculiarities, still continues under treatment."

In the table of supposed causes we find the following: "Millerism 1, spiritual rappings 1, religious excitement 2."

Small libraries have been established in some of the wards. "Pictures of a cheerful character hung on the walls, and mottoes suggestive of pleasant ideas, and printed in large letters, have been introduced into the wards, more particularly of the excited classes." The donations from Philadelphia, collected by Miss Dix, and mentioned in our notice of the report for 1852, amounted to $5,182. "The museum and reading-room buildings have been finished. They are 42 feet long, by 25 feet wide. A portico runs nearly the whole length of the front, from which a very pleasant view is obtained. They are placed one on either side of the front of the building; and each is easily accessible from the wards of the sex for which it is intended. It is proposed, so far as can be done, to procure the mineral and geological productions of the different parts of the Commonwealth, and to give to each County so much room as may be needed to exhibit the specimens obtained."

8. In the twelve months preceding the 30th September, 1853, the number of patients at the *Western Asylum of Virginia*, exceeded, by twenty-two, that of any preceding year. No malignant or epidemic disease occurred among them. Neither was there a case of suicide. Of the 1264 patients received since the opening of the Asylum, only five have terminated their existence with their own hands. In two of these there was no certainty that the death was not accidental.

	Men.	Women.	Total.
Patients at the beginning of the year	202	138	340
Admitted in course of the year	69	51	120
Whole number	271	189	460
Discharged, including deaths	54	29	83
Remaining at the end of the year	217	160	377
Of those discharged, there were cured	26	15	41
Died	17	10	27

The diseases terminating fatally are not reported. Among the causes of insanity, we observe, that the "excessive use of tobacco" is mentioned in *three* of the cases, "inhaling tobacco fumes" in *one*, and the "excessive use of tobacco and ardent spirits" in *one*.

Of the 460 cases, the insanity *commenced* before the age of 20 years, in 67; between 20 and 30, in 160; between 30 and 40, in 93; between 40 and 50, in 50; after the fiftieth year, 18; unascertained, 72. The great preponderance of the decennium from 20 to 30 years will be perceived.

	Men.	Women.	Total.
Aggregate of patients admitted since July 1, 1836	711	474	1185
Discharged cured	270	184	454
Died	150	86	236

Aside from the statistical tables, the report of Dr. Stribling is almost exclusively occupied in the description of improvements recently made upon the premises, and the suggestion of others. Gas was introduced for the purpose of lighting the apartments, on the 1st of January, 1853. "We are now satisfied," says the report, "that the institution can be supplied from the gas-works with an amount of light far greater than that which it formerly derived from oil, lard, and candles, *for a small fraction of what these materials cost.*"

As this establishment and the Eastern Asylum can accommodate but about 700 patients, and as there are within the State, according to the last census, 922 insane whites, and 945 white idiots, many of the latter probably not congenitally idiotic, the Doctor urges upon the Legislature "to make at once a liberal appropriation for the erection of another Asylum" for 250 patients.

9. On the 1st of July, 1852, Dr. S. Hanbury Smith retired from the superintendence of the *Ohio Lunatic Asylum*, and was succeeded by Dr. Elijah Kendrick. The reports heretofore emanating from this institution have been more voluminous than those from any other similar establishment in the country, with, perhaps, a single exception. The one now before us is of more restricted limits.

	Men.	Women.	Total.
Patients in the Asylum Nov. 15, 1851 . .	150	151	301
Admitted in course of the fiscal year . .	149	126	275
Whole number " " " . . .	299	277	576
Discharged, including deaths	169	147	316
Remaining, Nov. 15, 1852	130	130	260
Of those discharged, there were cured . .	70	71	141
Died	37	21	58

Deaths from consumption, 13; diarrhœa, 8; dysentery, 8; epilepsy, 4; marasmus, 4; maniacal exhaustion, 4; typhoid fever, 4; gastritis, 2; anæmia, 2; inanition, 2; suicide, 2; apoplexy, organic lesion of brain, caries of vertebræ, typhoid pneumonia, and erysipelas, 1 each.

Of the cases admitted, the mental derangement of 22 is ascribed to "religious anxiety," and that of 26 to "spirit rappings." In the latter class, Dr. Kendrick remarks, that, "the suicidal tendency is especially prominent, while the constant resting of the thoughts upon the scenes of an imaginary world renders it more difficult to attract attention to those of the real. Such cases, though recent, have proved more unfavourable than any others of the same class."

Thirty-eight of the patients received had previously been inmates of the Asylum and discharged *recovered*. *Sixteen* of them had been absent less than a year. When will the physicians of all our institutions for the insane report this item of their statistics?—an item of more importance than many which they regularly place before us.

Of the 275 persons admitted, the insanity commenced before the age of 20 years in 45; between 20 and 30 years, 94; 30 and 40 years, 65; 40 and 50, 41; 50 and 60, 23; 60 and 70, 6; 70 and 80, 1.

From the fact that one hundred and fifty-one applications for admission were rejected in the course of the year, we infer that Ohio is beginning sorely to feel the want of another hospital.

The bodies of deceased patients not reclaimed by their friends, have heretofore been privately interred. The funerals are now conducted openly, and in the presence of many of the patients. The circumstances which induced this change, and the results of the experiment are thus related:—

"On a visiting tour through the grounds my ear caught the following dialogue between two patients at work. Said A.: 'What disposition do you suppose is made of our bodies after death here?' B. replied: 'In my opinion, the doctors boil us up.' 'Very true,' continued A., 'that may be the fate of some, but my opinion is that many of us are taken to doctors' shops, to have our bones picked and stuck up to view as our bodies are here.' From that moment I was resolved, if possible, to dispel this mental delusion. Accordingly, on the first occurrence of a death, the chaplain, the officers, and assistants, accompanied by many of the male patients, followed the deceased to his final resting place. Here they were addressed by the chaplain, in language chaste and appropriate, in every way calculated to convince their understanding that not only were they fed, clothed, and cared for during their lives, but that, at their deaths, they should not be forgotten. The effect was strikingly impressive.

"In this first experiment we realized our highest hopes. Many who were denied the privilege, reproached us for not having permitted them to unite with their friends in rendering the last tribute of respect to a departed fellow-sufferer. We still observe all the rites and ceremonies due and proper on such occasions; take out at all times a large number of patients, both male and female, and nothing indecorous or disorderly has yet transpired to interrupt the

practice. So far as we are capable of judging, the influence has been salutary and controlling."

Dr. Kendrick does not give a very flattering description of the condition of the buildings in regard to the facilities for promoting the comfort and restoration of the inmates. The water-closets and bathing apparatus, "in plain terms, are a disgrace to the institution." There are no means of forced ventilation, and the patients' "sleeping apartments are not warmed." He recommends an appropriation of $33,800 to remedy these and other defects.

Report for 1853:—

	Men.	Women.	Total.
Patients remaining Nov. 15, 1852 . . .	130	130	260
Admitted in course of the fiscal year . .	110	129	239
Whole number " " " . . .	240	259	499
Discharged, including deaths	125	122	247
Remaining Nov. 15, 1853	115	137	252
Of those discharged, there were cured . .	71	62	133
Died	12	12	24

Causes of Death.—Phthisis pulmonalis, 7; inanition, 4; maniacal exhaustion, 3; typhus fever, 2; bilious remittent fever, 1; congestive fever, 1; variola, 1; pleuropneumonia, 1; paralysis, 1; ulceration of bowels, 1; exhaustion from journey, 1; suicide, 1.

A case of smallpox was "developed, under most inexplicable circumstances, in the male department," and every precaution was taken to prevent the propagation of the disease among the patients. No other case occurred until two months afterwards, when a female patient was attacked, had the disease mildly, and recovered. An endemic fever, of a mixed character, commenced among the inmates about the middle of July, attained its height about the middle of August, and continued, "sporadically," to the time at which the report was written. "Though commencing as a common bilious remittent, owing to the hospital tendency, after the first few days it assumed the typhoid or typhous type; and again, in the case of convalescents, at the end of two weeks, reassumed the remittent form. Among the patients there were 31 cases." These were mostly of males, and one of them ended fatally. There was, also, one case of congestive fever, of which the patient died. Of the 31, "through the renovating influences of physical disease, and the necessary remedial agents used for their recovery, 13 were restored to reason concurrently with their convalescence from the fever. Several were also much improved mentally, but again relapsed. In all the cases, even in the demented, the mind seemed more clear during the attack than when in usual physical health."

Sixteen cases of the fever occurred among the employees of the institution, one of them terminating in death. Dr. Kendrick attributed the disease to malaria rising from the "illy constructed sewers and most offensive cesspools," and from the earth thrown up in digging numerous ditches for steam and water-pipes through "the sub-soils charged with the accumulated impurities of years." The disease was treated, in its early stage, with alteratives, aperients, and diaphoretics; when typhoid symptoms arose, by the addition of tonics and stimulants, and, upon the reassumption of the remittent type, by anti-periodics and tonics.

Of the 239 patients admitted, 68 had suffered from former attacks of insanity. Among the "probable causes" of the disease, "religious excitement" ranks the highest in numbers, 32 being assigned to it. It is evident that the etiology of the disease is differently viewed by different physicians. Dr. Stokes, of the Mt. Hope Institution, asserts, in one of his late reports, that he has never seen a case clearly traceable to the cause in question. Eleven cases are attributed to "spirit rappings." "For some of these," says Dr. K., "my sympathies have been strongly awakened, and, though deprecating the impious folly, I cannot refrain from here entering my feeble protest against the *indiscriminate* commitment of such persons to lunatic asylums." He then quotes some medico-legal remarks of such tenor as to lead the reader to the inference that, in some

of the patients alluded to, there was no delusion or insanity, other than that which might exist in regard to the so-called "spiritual manifestations."

Forty-eight of the patients admitted had the suicidal propensity. Thirty-five of them had attempted self-destruction. Of the thirty-five, thirteen had recovered from their mental disease at the close of the year.

	Men.	Women.	Total.
Whole number of patients, 1839 to 1853, inclusive	1220	1135	2355
Discharged recovered	601	570	1171
Died	188	135	323

Diseases which proved Fatal.—"Exhaustion and general decay, without discoverable local lesion, 56; consumption, 52; dysentery, 31; diarrhœa, 30; epilepsy, 28; fever, 28; inanition, 26; apoplexy, 16; palsy, 9; inflammation of the lungs, 7; dropsy, 6; inflammation of the brain, 4; inflammation of the liver, 3; chronic inflammation of the peritoneum, 3; tabes mesenterica, 3; suicide, 3; inflammation of the pericardium, 2; inflammation of the stomach, 2; erysipelas, 2; organic lesion of the brain, 1; caries of the vertebræ, 1; chronic inflammation of the bronchia, 1; bilious colic, 1; ulceration of the bowels, 1; inflammation of the kidney, 1; cancer of the womb, 1; cutaneous cancer, 1; gangrene of the face, 1; exhaustion from journey, 1; confluent smallpox, 1; cause not assigned, 1."

The State Legislature has made appropriations for warming the buildings by steam, in connection with forced ventilation, and for the construction of an infirmary.

P. E.

Art. XII.—*Report of the Select Committee of the Senate of the United States on the Sickness and Mortality on board Emigrant Ships,* August 2, 1854. 8vo. pp. 147.

The present report is that of a select Committee appointed under an order of the Senate of the United States, passed on the 7th of December, 1853, "to consider the cause and the extent of the sickness and mortality prevailing on board of emigrant ships on the voyage to this country; and whether any and what legislation is needed for the better protection of the health and lives of passengers on board of such vessels." To which Committee were also referred a petition of the American Medical Association, and a petition of inhabitants of New York, in relation to the same subject.

The Committee appear to have given to the task entrusted to them, all that attention which its importance, as well with reference to the business interests of the country as to the cause of humanity at large, demands.

"With a view to the procuration of accurate data from which to form a correct judgment, a circular was prepared, containing fourteen questions relative to the police, and sanitary management of emigrant ships, to which specific answers, based as far as practicable upon experience, were requested. Copies of this circular were addressed and sent to members of the medical profession (whose opportunities of forming correct opinions on the subject had been furnished by experience, and therefore are entitled to consideration), to merchants, to navigators who had been engaged in the passenger trade, to persons connected with the collection of the customs, to presidents of benevolent societies for the relief of indigent emigrants, and to others throughout the country, and in the cities at which the landing of foreign passengers has chiefly taken place."

The answers received to these circulars were very numerous, and, in many instances, are drawn up with great ability, furnishing materials in abundance for a most interesting report; and such the one before us unquestionably is.

It is well known that the sickness and mortality that has prevailed, for many years past, on board of emigrant ships arriving at the different ports of the United States, has been, in many instances, to an extent truly appalling.

Nor is it at all surprising that typhus fever, and cholera, and smallpox should occur, in their most malignant forms, among the passengers on board of emigrant vessels, destroying, every year, many thousands during the voyage, and as many more subsequent to their arrival here, when we consider the impaired health, the squalid and depressed condition of a large number of the emigrants when they enter on board, the over-crowded condition of the vessels, the want of adequate ventilation, the too general inattention paid to cleanliness, the badly cooked food—itself, frequently, of an inferior quality—the neglect of exercise and of a proper sanitary police generally, throughout the voyage.

From the returns received by the Committee of the Senate, it appears that the percentage of mortality among the passengers on board of New York vessels has been considerably greater than among those on board of vessels trading to Boston and other ports.

"This," in the words of the report before us, "might have been, perhaps, expected, for a variety of reasons. New York being the great commercial emporium of the Union, passengers from every country in Europe have been induced to regard it as the point to which they should direct their courses. Hence, the huge structures, furnished by the enterprise of that great metropolis for the transportation of passengers, have been crowded to excess, and, as a necessary consequence, the causes of disease have existed on board of those vessels to a greater extent than on any other. In general, the percentage of deaths decreases in proportion as the number of passengers is less; and it is found that where passengers have been distributed in smaller numbers, disease and death have been less prevalent. The table (annexed to the report) shows that the smallest percentage of deaths has occurred on vessels from ports of Europe other than those of Liverpool, London, Bremen, Hamburgh, and Havre, which, being out of the great thoroughfares of commerce, have presented fewer attractions to the great mass of emigrants. The vessels from these ports, being less crowded, are more easily ventilated and kept clean, and present greater facilities for the proper preparation of the food of the passengers, and for their exercise in good weather."

The means by which the occurrence of disease and the consequent destruction of life among the passengers on board of emigrant vessels may be obviated are sufficiently evident. By a proper apportionment of the number of the passengers to the available accommodations afforded by each ship; by excluding from on board persons already diseased, and all foul and infectious baggage, as well as all individuals who are in a condition strongly predisposed to an attack of any contagious malady and liable to propagate it among the entire ship's company, in connection with the strict enforcement of a proper system of nautical hygiene throughout the voyage, the ratio of sickness and of deaths on board of emigrant vessels would be reduced even below that of the communities at home from which come the major portion of the emigrants to this country.

But while all who have paid any attention to the subject must be convinced as to the means best adapted to preserve the health and lives of emigrant passengers, there stands in the way of carrying these means into effect an almost insuperable obstacle. The enactment of the most precise and stringent regulations by the Congress of the United States would not be sufficient; the accomplishment of the important object aimed at by such regulations could not be attained unless they were legalized and enforced in every port in Europe where the embarkation of emigrants to this country takes place.

"The great difficulty," says the report before us, "of preparing a general law which shall embrace in its operation all the minute points involved in the management of passenger ships, and effectively place the passenger and ship owner on the best possible footing, must present itself to every mind. In the first place, our country being the general receptacle of emigrants from almost every country of Europe, it becomes necessary so to legislate as to avoid coming into conflict with the legislation of the countries whence these people come, and, at the same time, protect their interests and the interests of American citizens at home and abroad. Great Britain, France, the various nations of Germany, the Hanseatic cities, have all of them their passenger laws, in which they prescribe

the terms upon which those who emigrate from among them shall be conveyed across the ocean. These laws or regulations include not only the equipment and nautical management of the ships engaged in this trade, but also prescribe such dietary provisions as to their makers respectively appear essential to the health and comfort of passengers. To legislate in this country, therefore, so as not to come in conflict with these foreign enactments on some of the many minute points which present themselves in the treatment of such a subject, requires an extent and accuracy of information on details difficult to attain, and would involve a minuteness and variety of legislative enactments suited to the local requirements of each country, whence emigrants seek a home on our shores, and liable to become oppressive whenever a change of policy suggests a change in those foreign enactments. It is, of course, the intention of every law-maker, that the laws passed by him shall be enforced; and to place enactments on the statue book which cannot be carried into effect without subjecting those who are governed by them to difficulty and annoyance from foreign countries, is worse than useless. That such a state at present exists, under the passenger laws now in force, has been proved by experience; and those engaged in the transportation of passengers do not hesitate to say, that the laws under which they are acting are in many respects impracticable, unless at great expense and loss to the American ship-owner. It is only necessary to allude to one case of the kind by way of illustration. The acts of Congress require that every passenger ship shall be furnished with provisions of a certain description to a given amount. An American ship goes to Bremen or Hamburgh, and there takes on board a cargo of passengers. The laws of those cities require that all passenger ships sailing from them with passengers shall be supplied with a prescribed amount of certain provisions, which are specified. Now, in order to comply with the regulation of the port whence he sails for America, the American captain must provide a supply of the articles required by the local law, and, at the same time, to comply with the law of his own country, he must be provided with the food called for by the act of Congress. This single case involves a double expense in the provisions made for the subsistence of passengers."

To obviate the difficulty above referred to, the Committee have endeavoured to frame a law which shall, in its general operation, provide for the safety and comfort of passengers, and, at the same time, leave the management of minute details in the hands of those whose interest as well as business it is to be thoroughly acquainted with them. They consider it to be utterly impossible for Congress to know what is required to make each ship comfortable and healthy. Thus, one vessel may be ventilated and made perfectly wholesome for passengers by a process which, in another vessel, and under different circumstances, would be totally inadequate. The Committee have, therefore, believed it best to leave the means by which ships are made safe and comfortable mainly at the disposal of their owners; and, at the same time, to *make these responsible* for any untoward results that may attend the administration of their own affairs.

The Committee refer to a report on Quarantine, presented to the British Parlament, in 1849, in which the subject of the means for preventing sickness and death on board of passenger vessels are elaborately discussed. The Commissioners by whom this report was drawn up, Lords Carlisle and Ashby, and Edwin Chadwick and T. Southwood Smith, remark that, "the experience of the mercantile navy itself affords an example of the successful working of a large preventive principle of jurisprudence; that is to say, *the principle of concentrating responsibility on those who have a direct interest in prevention, and who possess the best means of securing it.* We would request attention to those examples for international consideration."

The report referred to then goes on to say:—

"It is stated that when the system of transportation was first adopted in some of the earlier voyages, full one-half of those who embarked were lost; later, on the passage to New South Wales, as in the 'Hillsborough,' out of three hundred and six who embarked one hundred were lost; and in another ship, the 'Atlas,' out of one hundred and seventy-five embarked sixty-one were

lost. Yet there were no omissions palpable to common observation, or which could be distinctly proved as matter of crimination to which responsibility might be attached. The shippers were, no doubt, honourable men, chargeable with no conscious designs against the lives of the human beings committed to their care, and with no unusual omissions; but their thoughts were directed, by their interests, exclusively to profits; they got as much freight as they could, and they saw no reason why convicts or emigrants should not put up with temporary inconveniences to make room for cargo.

"By a simple change—based on the principle of self-interest, the most general, uniform, and, when properly directed, really beneficent of all principles of action—by the short alteration of the terms of the contract, so as to apply the motive where alone there was the effectual means of prevention, by engaging *to pay for those landed alive*, instead of paying for *all those embarked*, these extreme horrors were arrested; the generation of extensive mortal epidemics was, in a short time, prevented, and clean bills of health might have been given to all the ships which before would have been entitled to none. From the report of the select committee on transportation, in the year 1812, it appears that in one period, namely, from 1795 to 1801, out of three thousand eight hundred and thirty-three convicts embarked three hundred and eighty-five died, being nearly one in ten. But, since 1801, after the principle of responsibility began to be applied, out of two thousand three hundred and ninety-eight embarked only fifty-two have died, being one in forty-six. The improvement has continued up to the present time, when it amounts to only one and a half per cent., or even lower than the average mortality of such a class living on shore. The shippers themselves, without any legislative provisions, or any official supervision or regulations thereto, appointed medical officers or surgeons, and put the whole of the convicts under their charge; the shippers attested their own sense of the propriety, sound policy, and efficiency of the principle, by voluntarily adopting it and applying it to each ship-surgeon in charge, whose remuneration was made dependent upon the number of passengers landed alive."

The same principle, we are told, has been adopted in contracts for the transportation of troops, and found to operate equally well; also to the transportation of pauper emigrants, with a like result; and the authors of the report to Parliament think the adoption of it will become general in the transportation of passengers, as it puts an end to all the difficulties occurring between the passengers and the officers of the ship, who are, of course, disposed to remain on the best of terms with those whom they carry.

"Why, then," asks the committee whose report is before us, "cannot or should not this principle of self-interest, as universal as the existence of man, be adopted in the transportation of passengers generally? As the law stood formerly, wages could not be demanded in case of shipwreck; and even now it must be made to appear that every effort has been made by the officers and crew to save the ship, before a claim for wages can be sustained."

"Is there any sufficient reason why similar responsibility should not be thrown upon those who have the entire management of the ship and the means of keeping it in proper condition? Should merchandise be on a better footing than human life? And if there be any propriety or justice in the old common law maxim, that freight was the mother of wages, why should not the safe delivery of the passengers at the port of destination be the foundation of any claim for passage-money? The committee can see none, and have therefore adopted the principle, and regarded it as, perhaps, the most important feature in the bill which they recommend to be enacted into a law. If they are not very much mistaken, the change here made in the relation between the shipper and the passenger will bring about an entirely new era in the history of passenger-ships; as it will be the interest of shippers to land their passengers in safety, so it will be their pleasure to provide their ships with whatever may be necessary to make them healthy and comfortable. Nor will there hereafter be the same carelessness in taking enfeebled, broken-down, or diseased passengers on board, as has heretofore existed; shippers will be cautious in seeing, at all events, that the individual, of whose life during the passage he is to become virtually the insurer, is not labouring under serious sickness at the time of embarkation.

The adoption of this principle will further have a most powerful influence in creating a rivalry between ships as to which is the cleanest and healthiest; passengers will always prefer those which have the reputation of being the most lucky, or, in other words, the cleanest, best managed, and affording the greatest comfort to those who embark in them. Nor will the ventilation, disinfection, &c. of these vessels hereafter require the interference of Congress; each ship-master will be as desirous to have his ship sweet, wholesome, and pleasant in its arrangements, as the hotel-keeper who desires to attract patronage by the superior neatness, cleanliness, and good order of his establishment. The owners of steamers on the noble rivers of our country, and stage-coaches on its great thoroughfares, require no legislation in regard to the spaciousness and airiness of their apartments, or the good driving and safety of construction of the other, simply because self-interest dictates to them the comfort and security of the travelling public as the best means to secure custom. So will it be with the owners of passenger-vessels when left to themselves; and they will provide for the health and comfort of the poor emigrant as well as of the millionaire, because, in doing so, they will protect their purses and their own interests."

The following is an outline of the measures recommended by the committee, after a careful examination of the subjects committed to their consideration:—

1. That a space be reserved on the upper deck, and kept clear, for the enjoyment of air and exercise by the passengers. If space cannot be allowed for the accommodation of all the passengers at once, they to be divided into squads, and take their exercise in turn at certain hours of each day should the weather permit. The committee very correctly remark, that, "nothing is more conducive to the cure of sea-sickness, and the preservation of the strength and good spirits of passengers at sea, than the necessity of moving about and becoming interested in what is going on around them. At the same time, the temporary absence of passengers from the sleeping apartments will afford an opportunity for cleansing them, and removing whatever may be offensive or injurious to health."

2. That the former limitation of two passengers to every five tons register be restored, but without diminishing the space allowed under the present law to each passenger, to prevent the crowding, that now takes place, of vast numbers in any one ship.

3. That during the winter the number of passengers be limited to one for every three tons, as the inclemency of the weather and consequent necessity of keeping the hatches closed obstruct ventilation, and prevent the passengers from taking exercise in the open air.

4. That no passengers be allowed to occupy the orlop deck, which, lying next the kelson, and entirely under water, must, it is very evident, be almost entirely deprived of air and light.

5. That the number of privies be increased, with separate ones for the females. As the committee remark, the present allowance of one to every hundred passengers, is totally inadequate. "Common decency would seem to indicate the propriety, or rather necessity, of having separate accommodations for females, whose health and comfort are often destroyed by the fear of exposure to the brutal remarks of the vulgar and obscene among the male passengers and the crew."

6. That, from the entire impossibility of hundreds of people, some of them feeble from sickness, doing their cooking in a proper manner, at a caboose four feet by one and a half in size, the provisions be cooked as well as furnished by the ship.

7. That, with a view to confer upon the captain ample power to maintain discipline, without being subject, as at present, to vexatious prosecutions, certain rules be adopted and published throughout the ship, the observance of which to be enforced by the master in case of resistance to his proper orders and authority; a statement of the facts to be entered at the time on the log, and signed by the surgeon—if any be on board—and mate, and read to the offender. The statement so made to be *prima facie* evidence of justification in all suits or complaints brought against him, or against any who aid in carrying out his orders.

8. That the ship be made responsible to the extent of the passage-money, in the event of death during the passage.

9. That a full and accurate return be made of the names and descriptions of passengers, and of the deaths on board.

These regulations, so far as they go, appear perfectly judicious, and, with the requirement that in every case the shipper shall become virtually the insurer of the passenger's life during the voyage, would no doubt be sufficient to put a stop to the excessive amount of sickness and mortality which has heretofore prevailed on board the majority of the emigrant vessels arriving in the ports of the United States.

This able report, drawn up, we believe, by the Hon. Hamilton Fish, of New York, is in the highest degree creditable to the industry, intelligence, and philanthropy of this eminent statesman, and it is to be hoped that Congress will, at the earliest opportunity, carry out the measures recommended.

D. F. C.

Art. XIII.—*Puerperal Fever, as a Private Pestilence.* By Oliver Wendell Holmes, M. D., Parkman Professor of Anatomy and Physiology in Harvard University. 8vo. pp. 60. Boston, 1855. Ticknor & Fields.

On the Induction of Puerperal Fever, by Inoculation, so called. By Z. Pitcher, M. D. Published at the request of the Detroit Medical Society. Peninsular Journal of Medicine for February, 1855.

That puerperal fever, so called, is not, strictly speaking, a disease peculiar to the parturient female; that, under circumstances, of by no means unfrequent occurrence, an affection, attended by the same general symptoms, and in which, after death, the same morbid lesions are found to exist, is liable to occur in the virgin and non-pregnant female, are positions now so well established that they can scarcely admit of a reasonable doubt.

The close relationship between puerperal fever and erysipelas is another fact we believe to be clearly and incontestably proved. As remarked, long since, by Dr. Gordon, of Aberdeen, the two diseases prevail, most generally, as concomitant epidemics. They begin at nearly the same time, keep pace with each other, and cease simultaneously, or nearly so. It would require more space than it would be convenient to devote to a single bibliographical notice to present even a brief abstract of the clear and positive evidence in support of this position, accumulated in the records of our profession.

There is still another fact in relation to this disease which appears to us to be as unquestionably established, as either of the preceding; namely, that puerperal fever may be induced by the introduction into the vagina or uterus, during or subsequent to parturition, by the hands of the accoucheur, a virus derived from patients affected with erysipelas, or from the bodies of those who have died of that disease or of erysipelas.

We are inclined to believe that another fact may be predicated of erysipelas and puerperal fever; namely, that both diseases are propagated by contagion; and that the two are capable of reciprocally producing each other. It is true, that the contagious character of erysipelas and puerperal fever is by no means so positively established as either of the preceding positions. When a disease, as is most commonly the case with both these, prevails as an epidemic or endemic, it is not always an easy matter to determine, with certainty, whether its spread is due to the general morbific condition of the atmosphere, which unquestionably prevails at the time, or to a poison, received either directly or indirectly, from the bodies of the sick. Still, there are on record a long series of well-authenticated facts which would seem very certainly to establish the contagiousness of both the diseases under consideration, and many have fallen under our own immediate observation in reference to the spread of childbed fever which, we confess, we should find very difficult to explain in a satisfactory manner without admitting the contagious character of the disease.

Dr. Holmes has collected and arranged all the leading facts in proof of this

side of the question adduced by different observers, previously to the year 1843. The character of these facts, and the sources from which the majority of them are derived, press them strongly upon the attention of those engaged in the practice of obstetrics, as well as of the profession generally. Their force cannot be evaded by the merely negative experience of any obstetrician, or by an attempt to disparage the professional standing, and the logical acumen, of those by whom they are recorded, or by underrating their opportunities for accurate observation. They demand a cautious, candid examination. If they can be fairly explained in accordance with the hypothesis that puerperal fever is a simple phlegmasia of the reproductive organs and peritoneum in the parturient female, and that it is not capable of being communicated from those labouring under the disease to any other female in childbed; let such explanation be given. Nay, if the supposed facts can be proved to be the result of inaccurate, or imperfect, or one-sided observations, let this be done; but, at the same time, let it be always borne in mind that it is with the asserted facts themselves we have to deal, and this fairly, candidly, and logically. If it can be shown that they are inadequate to sustain the doctrine they have been adduced to support, good and well. But if this cannot be done, they may be sneered at, their authors denounced as simpletons, and those who believe in their validity as dupes, but the facts themselves cannot by this means be erased from the records of our profession, nor until they are refuted, or the interpretation given to them shown to be erroneous, can their force be evaded.

The essay of Dr. Holmes, the title of which is placed at the head of this article, was first published some twelve years ago. The edition before us, although, so far as the essay itself is concerned, an exact reprint, "without the change of a word or syllable," of the first, we have esteemed worthy of a special notice, as well from the deep, the painful interest which every practitioner must feel in the important subject discussed in it, as from the very piquant introduction with which the author has thought proper to accompany it, and in which he has handled "with ungloved hands," such as deny the slightest validity to, or obstinately close their eyes against, all observations, no matter how extended the series, or respectable the sources from which they emanate, that militate against any hypothesis they shall think proper to put forth as truth; as well as those who most illogically attempt to set at naught positive by mere negative testimony, finished facts by such as are void of completeness.

The essay of Dr. Holmes is pronounced by Ramsbotham, to be "the best paper, in any language, with which he is acquainted, written to prove the highly contagious nature of puerperal peritonitis. A masterly performance, and well worth perusal by any sceptics on the subject." It demands the candid attention of every one who feels an interest, and what physician does not? in one of the most important questions connected with the etiology of one of the most important diseases to which the child-bearing woman is liable. Let no one who has not made himself master of the facts embraced in the essay under consideration, who has not subjected them to a careful and candid analysis with the view of testing their true bearing upon the question at issue, attempt to dogmatize in reference to the contagious or non-contagious character of puerperal fever. Ample as his own individual experience may be, the results of that experience must be tested by the positive experience of all other observers within his reach, before it can be made the basis of any general law in either pathology or therapeutics.

In his introduction, Dr. Holmes, after a reference to Dr. Meigs' recent work on childbed fever, remarks:—

"One unpalatable expression, I suppose, the laws of construction oblige me to appropriate to myself, as my reward for a certain amount of labour bestowed on the investigation of a very important question of evidence, and a statement of my own practical conclusions. I take no offence and attempt no retort. No man makes a quarrel with me over the counterpane that covers a mother and her new-born infant at her breast. There is no epithet in the vocabulary of slight and sarcasm that can reach my personal sensibilities in such a controversy. Only just so far as a disrespectful phrase may turn the student aside from the examination of the evidence, by discrediting or dishonouring the witness, does it call for any word of notice.

"I appeal from the disparaging language by which the Professor in the Jefferson School of Philadelphia would dispose of my claims to be listened to. I appeal, not to the vote of the Society for Medical Improvement, although this was an unusual evidence of interest in the paper in question, for it was a vote passed among my own townsmen; nor to the opinion of any American, for none know better than the professors in the great schools of Philadelphia how cheaply the praise of native contemporary criticism is obtained. I appeal to the recorded opinions of those whom I do not know and who do not know me, nor care for me, except for the truth that I may have uttered; to Copland, in his Medical Dictionary, who has spoken of my essay in phrases to which the pamphlets of American 'scribblers' are seldom used from European authorities; to Ramsbotham, whose compendious eulogy is all that self-love could ask; to the Fifth Annual Report of the Registrar-General of England, in which the second-hand abstract of my essay figures largely, and not without favourable comment, in an important appended paper. These testimonies, half forgotten until this circumstance recalled them, are dragged into the light, not in a paroxysm of vanity, but to show that there may be food for thought in the small pamphlet which the Philadelphia teacher treats so lightly. They were at least unsought for, and would never have been proclaimed but for the sake of securing the privilege of a decent and unprejudiced hearing.

"I will take it for granted that they have so far counterpoised the depreciating language of my fellow-countryman and fellow-teacher, as to gain me a reader here and there among the youthful class of students I am now addressing. It is only for their sake that I think it necessary to analyze, or explain, or illustrate, or corroborate any portion of the following essay. But I know that nothing can be made too plain for beginners, and as I do not expect the practitioner, or even the more mature student, to take the trouble to follow me through an introduction which I consider wholly unnecessary and superfluous for them, I shall not hesitate to stoop to the most elementary simplicity for the benefit of the younger student. I do this more willingly, because it affords a good opportunity, as it seems to me, of exercising the untrained mind in that medical logic which does not seem to have been either taught or practised in our schools of late, to the extent that might be desired.

"I will now exhibit, in a series of propositions reduced to their simplest expression, the same essential statements and conclusions as are contained in the essay, with such commentaries and explanations as may be profitable to the inexperienced class of readers addressed:—

"I. It has been long believed by many competent observers, that puerperal fever (so called) is sometimes carried from patient to patient by medical assistants.

"II. The express object of this essay is to prove that it is so carried.

"III. In order to prove this point, it is not necessary to consult any medical theorist, as to whether or not it is consistent with his preconceived notions that such a mode of transfer should exist.

"IV. If the medical theorist insists on being consulted, and we see fit to indulge him, he cannot be allowed to assume that the alleged laws of contagion, *deduced from observation*, in other diseases, shall be cited to disprove the alleged laws *deduced from observation* in this. Science would never make progress under such conditions. Neither the long incubation of hydrophobia, nor the protecting power of vaccination, would ever have been admitted, if the results of observation in these affections had been rejected as contradictory to the previously ascertained laws of contagion.

"V. The disease in question is not a common one; producing, on the average, about three deaths in a thousand births, according to the English registration returns which I have examined.

"VI. When an unusually large number of cases of this disease occur about the same time, it is inferred, therefore, that there exists some special cause for this increased frequency. If the disease prevails extensively over a wide region of country, it is attributed without dispute to an *epidemic* influence. If it prevails in a single locality, as in a hospital, and not elsewhere, this is considered proof that some *local* cause is there active in its production.

"VII. When a large number of cases of this disease occur in rapid succession, in one individual's ordinary practice, and few or none elsewhere, these cases appearing in scattered localities, in patients of the same average condition as those who escape under the care of others; there is the same reason for connecting the cause of the disease with the *person* in this instance, as with the *place* in that last mentioned.

"VIII. Many series of cases, answering to these conditions, are given in this essay, and many others will be referred to which have occurred since it was written.

"IX. The alleged results of observation may be *set aside;* first, because the so-called facts are in their own nature equivocal; secondly, because they stand on insufficient authority; thirdly, because they are not sufficiently numerous. But, in this case, the disease is one of striking and well-marked character; the witnesses are experts, interested in denying and disbelieving the facts; the number of consecutive cases in many instances frightful, and the number of series of cases such, that I have no room for many of them except by mere reference.

"X. These results of observation, being admitted, may, we will suppose, be *interpreted* in different methods. Thus the coincidences may be considered the effect of *chance.* I have had the chances calculated by a competent person, that a given practitioner, A, shall have sixteen fatal cases in a month, on the following data: A, to average attendance upon two hundred and fifty births in a year; three deaths in one thousand births to be assumed as the average from puerperal fever; no epidemic to be at the time prevailing. It follows, from the answer given me, that if we suppose every one of the five hundred thousand births of England to have been recorded during the last half century, there would not be one chance in a million million millions, that one such series should be noted. No possible fractional error in this calculation can render the chance a working probability. Applied to dozens of series of various lengths, it is obviously an absurdity. Chance, therefore, is out of the question as an explanation of the admitted coincidences.

"XI. There is, therefore, *some* relation of cause and effect, between the physician's presence and the patient's disease.

"XII. Until it is proved to what *removable condition* attached to the attendant the disease is owing, he is bound to stay away from his patients so soon as he finds himself singled out to be tracked by the disease. How long and with what other precautions, I have suggested, without dictating, at the close of my essay. If the physician does not at once act on any reasonable suspicion of his being the medium of transfer, the families where he is engaged, if they are allowed to know the facts, should decline his services for the time. His feelings on the occasion, however interesting to himself, should not be even named in this connection. A physician who talks about *ceremony,* and *gratitude,* and the *treatment he got,* surely forgets himself; it is impossible that he should seriously think of these small matters where there is even a question whether he may not carry disease, and death, and bereavement into any one of his families, as they are sometimes called."

The leading object of Dr. Pitcher's paper is to disprove the communicability of puerperal fever by any cause connected with the person of the accoucheur, or with that of any of the attendants upon the parturient female. The question is one of absorbing interest, and certainly demands a close and candid examination. We do not think, however, that Dr. Pitcher has met it fully and fairly. He has not investigated or collated the numerous and startling facts that have been adduced in support of the affirmative side, while he has attempted to establish the negative simply by a reference to certain generalities, which, though in themselves correct, have very little weight in the settlement of the point at issue.

Dr. Pitcher admits "that epidemic puerperal fever is an erysipelatous inflammation," that it "may be the occasion of abortion, of premature labours; that it is often developed before parturition, and still more frequently excited into activity by the accidents of labour." That in "a majority of cases" it is "essentially a traumatic erysipelas."

While he recognizes the frequent dependence of the disease upon an epidemic cause, he admits, also, that, within a certain limitation, its spread is due to a special contagion. The sphere of activity of this contagion is, however, he maintains, very limited; it may, he thinks, multiply the cases of puerperal fever among the inmates of the same lying-in ward, but beyond this is perfectly harmless. If, he remarks, the disease were of an unqualifiedly contagious or infectious character it should be communicable "to the pregnant as well as the parturient female, and why not as well to persons of either sex or any condition?"

Dr. Pitcher repudiates the idea of the propagation of puerperal fever "by the touch of the accoucheur." "How," he remarks, "so grave a disease can be transplanted by the touch of a person in perfect health, who has had no communication with the disease for days, and, perhaps, weeks, is as difficult for him to comprehend as that the royal touch should heal the scrofula."

Puerperal fever is either contagious or not contagious; if contagious, as Dr. Pitcher admits, it must be so absolutely; we have no knowledge of a contingent, occasional, or partial contagion. It is true, that the contagion of puerperal fever, like that of every other contagious disease, is active only within a certain circumscribed sphere, and the intensity of its action is augmented in close, crowded, and ill-ventilated apartments. We are to remember, however, that the contagious matter of disease is capable of adhering to the persons and clothing of individuals; to the bedding, and, also, it is probable, to the walls, etc., of rooms that have been occupied by the sick; in this manner smallpox and typhus fever have been communicated to persons who had never been in contact with patients labouring under these diseases, and in places remote from those in which the contagion was generated.

That the persons and clothing of individuals may become the media by which contagion is conveyed so as to produce disease, while the individuals themselves exhibit no indications of sickness, is proved by the occurrences at the Oxford assizes, in 1577, at those of Exeter and Taunton in 1586, and at those of the Old Bailey in 1736 and 1750; and by similar occurrences in various lazarettoes, barracks, hospitals, and transport and passenger vessels. How long the contagious matter may thus adhere to the person and clothing without losing its power to generate disease, it is impossible to say, in the absence of any accurate series of observations, undertaken to test the question. It is very certain that the contagion of puerperal fever is of a particularly adhesive and persistent nature. It has been found extremely difficult to expel it from the ward of a lying-in hospital in which the fever has prevailed. Even after the apartment has been vacated, thoroughly cleansed, and exposed to the freest ventilation for many weeks, immediately upon its being re-occupied it is no uncommon thing for the disease again to break out among its inmates, while no cases of it occur elsewhere.

Dr. Pitcher thinks that if puerperal fever is unqualifiedly contagious it should be communicable to the pregnant as well as the parturient female; to persons of either sex and every condition. Now we believe that it is actually thus communicable. We think that there can be no doubt that the affection denominated puerperal fever, and certain forms of erysipelas are in their true pathological character identical; generated by the same endemic or epidemic cause, and capable, by a contagion, of reproducing each other, reciprocally. During the prevalence of epidemic erysipelas, peritonitis is an ordinary concomitant in both men and women, while all the lesions, usually detected in cases of death from puerperal fever, are frequently met with in the virgin as well as in the pregnant and parturient female. We have reported, from a great variety of sources, cases, almost innumerable, of the production of puerperal fever by the contagion of erysipelas, while, on the other hand, instances are detailed on unquestionable authority; among others, we would instance those of Mr. Robert Storrs, of Doncaster, England; in which the contagious effects of puerperal fever on the male subject, as well as on non-parturient persons, was evinced; actual contact or close approximation with patients labouring under childbed fever, producing inflammation of the peritoneum or other serous membranes, accompanied by low fever, both in the male and female subject; erysipelas, local or

general, in both males and females; and typhus fever, with its various accompaniments, and in a variety of forms.

That the parturient female should be more subject to the influence of the epidemic constitution of the atmosphere productive of that peculiar pathological condition in which, we presume, both erysipelas and childbed fever essentially consist, and that, in her, the lesions should be chiefly detected in the pelvic viscera is not at all surprising.

But it is not only by contagion, in the proper sense of the term, that puerperal fever may be propagated, but it may also by direct inoculation; by the hands of the accoucheur introducing into the vagina or uterus a virus derived from patients labouring under severe forms of erysipelas, or in examinations of the bodies of those who have died of peritonitis, or phlebitis, or, probably, from the diseased secretions of the vagina in certain cases of childbed fever. The possibility of the induction of puerperal fever in this manner is very fully established by the experience of the physicians having charge of the lying-in departments in the general hospital of Vienna; where, according to the statement of Dr. Routh, by taking measures to prevent the possibility of inoculation in the manner referred to, the disease, which, previously, had prevailed to a fearful extent, has become of comparatively rare occurrence.

Dr. Pitcher having been led by numerous examples, most of which have occurred in his own practice, to adopt the belief "that puerperal fever is, in a majority of cases, essentially a traumatic erysipelas," the predisposition to, and approach of which may be foreseen and guarded against; consequently repudiates the idea of its propagation by the touch of the accoucheur, and he "holds it to be fallacious to resort to such extraordinary modes of explaining phenomena that may be accounted for, on the same principles that are applicable and daily appealed to, in speaking of the spread of other epidemic diseases, such as influenza, typhoid fever, and the dengue of the south, all admitted to be dependent upon a peculiar atmospheric constitution, and, in the opinion of many, spread by the influence of a special contagion."

It is true, that in numerous instances it is scarcely possible to determine with certainty whether the spread of puerperal fever is due to contagion, or is referable to a peculiar morbific condition of the atmosphere of a particular district, or town, or hospital; but, there are, not unfrequently, particular circumstances connected with the spread of the disease which cannot be accounted for upon, but are directly opposed to, the supposition that it is due, in every instance, to a general or local atmospheric cause; while, at the same time, facts are adduced which go absolutely to prove that its propagation is, in some instances at least, caused by contagion or inoculation. In this it resembles many of, if not all, the other known contagious affections, which may originate from epidemic or endemic causes, and be propagated by contagion, or communicated by inoculation.

D. F. C.

ART. XIV.—*A Treatise on the Practice of Medicine.* By GEORGE B. WOOD, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia; One of the Physicians of the Pennsylvania Hospital; One of the Authors of the Dispensatory of the United States of America, etc. etc. Fourth Edition. 2 vols. 8vo. pp. 852, 864. Philadelphia, 1855. Lippincott, Grambo and Co.

THE announcement of a new edition of Dr. Wood's treatise on the practice of medicine may be made in a very few words. We take it for granted that there are very few physicians in the United States who are not by this time perfectly familiar with the work, or, who have not formed for themselves a just estimate of its merits.

We know of no treatise which presents a more full and accurate synopsis of the present state of our knowledge, in regard to the pathology of the leading ailments incident to the human organism: no one that can, with greater safety,

be placed into the hands of the student as his guide to correct views in reference to the nature, causation, symptoms, and march of disease, and to sound practical directions for its treatment; no one that may be consulted, as occasion demands, by the practitioner, with greater profit and satisfaction.

Devoid of all pretension in his teachings, Dr. Wood's sole object in the preparation of the present treatise was evidently "to present, along with the results of his own personal observation, experience, and reflection, in reference to the practice of medicine, such a view of this important department of medical science, as fairly to represent its general condition at the time." And this object, all who have studied the work with candour and attention, will acknowledge, has been kept constantly in view by the author, and very successfully attained.

We meet, in its pages, with no straining after the questionable character of originality by the enunciation of novel and startling theories, or of attractive, but misleading generalities, nor any attempt to build up an exclusive plan for the treatment of disease, by rejecting with contempt, or ignoring the accumulated and concurrent experience of his predecessors or contemporaries. But, in a concise, perspicuous, unornate style, Dr. Wood delineates the broad outlines of the theory and practice of medicine, as recognized and illustrated by the most reliable and recent of the authorities in our profession.

In the present edition is incorporated a faithful digest of whatever may be esteemed true and useful, that has been developed since the appearance of the former one, in reference to either of the subjects embraced within the general scope of the treatise; it will be found, therefore, fully brought up to the present time.

D. F. C.

Art. XV.—*The Principles and Practice of Obstetric Medicine and Surgery in Reference to the Process of Parturition.* With sixty-four plates, and numerous wood-cuts. By Francis H. Ramsbotham, M. D., Fellow of the Royal College of London; Obstetric Physician to the London Hospital, and Lecturer on Obstetric and Forensic Medicine at the London Hospital Medical College, etc., etc. A new American Edition, revised by the Author. With Notes and Additions, by William V. Keating, M. D., A. M., Lecturer on Obstetrics and Diseases of Women in the Philadelphia Medical Association, Physician to the St. Joseph's Hospital, Member of the American Medical Association, Fellow of the College of Physicians, Member of the American Philosophical Society, etc. etc. *Τὰ μὲν ἄλλα καὶ πόλεμος καὶ μεταβολὴ τύχης ἀνάλωσαν. ἡ Τέχνη δὲ σώζεται.* 8vo. pp. 648. Philadelphia, 1855, Blanchard and Lea.

The work of Dr. Ramsbotham has been now for many years before the medical public, and has received the almost unanimous approval of the professional periodical press for the clear and accurate demonstrations it imparts of the doctrines of obstetrical science, and the sound practical precepts it inculcates. It cannot fail always to command a high rank among the numerous treatises which have, of late years, been presented as guides to the student in the acquisition of the necessary elementary knowledge of the principles and practice of obstetrics, and as books of reference for the practitioner who desires to refresh his memory on the leading points connected with the rules and practical details of his profession as recognized by the best authorities of the day.

Sufficiently full in its details, while in its style it is clear and concise, it is, at the same time, plain and unpretending, but sound and explicit in the practical instructions it inculcates, and cautious in the exposition it presents of the particular conditions under which manual or instrumental interference is demanded in the conduct of labor, with reference to the safety of the mother or child, or of both. Every important point connected with the anatomy of the organs concerned in child-bearing, and with the changes they undergo during

gestation—the several positions of the fœtus in utero, the mechanism of labour, and every subject of practical importance connected with obstetrics, are illustrated by a series of accurate and well executed engravings, admirably calculated to render them clear to the comprehension, and, at the same time, to fix them upon the mind of the student.

As an exponent of British Midwifery, the work of Dr. Ramsbotham is among the best extant, and, for the admirable synopsis it presents of the several subjects embraced within its general plan; the good common sense which pervades its doctrines and its precepts, and the plain, unpretending practical knowledge it inculcates, it may be safely recommended as one admirably adapted for the use of the student and junior practitioner.

The present edition has received the especial revision of the author with the view to its publication in this country, while the Editor has endeavoured to "adapt the work more fully to the wants of the American Practitioner," by embodying, as far as possible, the peculiar views of the most eminent of our own authorities in obstetrics.

The additions he has made, "refer chiefly to the inclined planes of the pelvis; generation; signs of pregnancy; fœtal circulation; mechanism of labour; employment of anæsthetics; use of ergot; American forceps and rules for their application; views of American Obstetricians on puerperal fever; Professor Clark's treatment of puerperal fever; rupture of the uterus; causes of abortion, etc."

These additions are, upon the whole, judicious, and enhance the value of the work to the American student and practitioner.

In regard to its mechanical execution, the present edition is worthy of all praise. The typography and illustrations are of the best style as regards accuracy as well as elegance. The volume will form a beautiful, as it unquestionably will a useful addition to the library of the American obstetrician.

D. F. C.

ART. XVI.—*On Criminal Abortion: a Lecture introductory to the Course on Obstetrics and Diseases of Women and Children in the University of Pennsylvania, Session of* 1854–'5. By HUGH L. HODGE, M. D. 8vo. pp. 20.

WE have read, with no little satisfaction, this very excellent introductory lecture. It is dignified in manner, high-toned in its sentiments, and inculcates, with becoming earnestness, the obligation of the practitioner of any and all the branches of the healing art, not only to perform with skill and faithfulness his merely professional offices, but to observe strictly towards his patients, and society generally, a code of the purest ethics. The lecture, throughout, is one well calculated to impress upon the minds of those who are preparing themselves to enter the ranks of our profession, to practise in either of its departments, a sense of their deep responsibilities and incumbent moral obligations.

It is, however, to the leading theme of the lecturer—criminal abortion—that we would particularly call attention on the present occasion.

In reference to this execrable practice, which, we have reason to believe, is pursued in our very midst to an extent of which many have no conception, Professor Hodge speaks with a plainness of language and warmth of indignation that does him much credit. If his language and feelings, in this respect, shall be responded to by the profession generally, throughout the breadth and length of our land, we shall, at least, absolve ourselves from all participation in the guilt incurred by those who, having gained admittance into our ranks, are willing, for a price, to prostitute themselves as panderers to vice by the criminal induction of abortion; while, at the same time, we may be the means of diminishing the frequency of the crime, if we shall be unable entirely to suppress it.

There can be no doubt that, in a proper ethical, if not in a legal sense, Judge King, of Philadelphia, was strictly correct when he declared "every act of abor-

tion to be murder, whether the individual perpetrating such act intended to kill the woman, or merely feloniously to destroy the fruit of her womb." It is certainly "a base and unmanly act"—"a crime against the natural feelings of man—against the welfare and safety of females—against the peace and prosperity of society—against the divine command, 'Thou shalt not kill.' It is murder.

"It is in vain to evade this conclusion. As far as human investigation has gone, or probably ever will go, in penetrating the mysterious functions of generation—as far as the light of reason or the torch of Revelation has elucidated the subject, there can be no reasonable doubt that human existence, corporeally and spiritually, commences, not with the birth of the fœtus and the first inspiration, but at conception, when the germs furnished by both parents are quickened into life." By what strange perversion of the moral sense has mankind come so generally to view the intentional destruction of the life of the fœtus prior to birth as a venial offence, and to render this form of murder so prevalent among the most intelligent, refined, moral, and Christian communities?

"We blush," says Dr. Hodge, "while we record the fact that, in this country, in our cities and towns, in this metropolis, where literature, science, morality, and Christianity are supposed to have so much influence—where all the domestic and social virtues are reported as being in full and delightful exercise—even here individuals, male and female, exist, who are continually imbruing their hands in the blood of unborn infants; yea, even *medical* men are to be found who, for some trifling pecuniary recompense, will poison the fountains of life, or forcibly induce labour, to the certain destruction of the fœtus, and not unfrequently of its parent.

"So low is the moral sense of the community on this subject, so ignorant are the greater number of individuals, that even mothers, in many instances, shrink not from the commission of this crime, but will voluntarily destroy their own progeny, in violation of every natural sentiment, and in opposition to the laws of God and man. Perhaps there are few individuals in extensive practice as obstetricians, who have not had frequent applications made to them, by fathers and mothers of unborn children (respectable and polite in their general appearance and manners), to destroy the fruit of illicit pleasure, under the vain hope of preserving their reputation by this unnatural and guilty sacrifice.

"Married women, also, from the fear of labour—from indisposition to have the care, the expense, or the trouble of children—or some other motive, equally trifling and degrading, have solicited that the embryo should be destroyed by their medical attendant. And when such individuals are informed of the nature of the transaction, there is an expression of real or pretended surprise that any one should deem the act improper, much more guilty; yea, in spite even of the solemn warning of the physician, they will resort to the debased and murderous charlatan, who, for a piece of silver, will annihilate the life of a fœtus, and endanger even that of its ignorant and guilty mother.

"This low estimate of the importance of fœtal life is by no means restricted to the ignorant, or to the lower classes of society. Educated, refined, and fashionable women—yea, in many instances, women whose moral character is in other respects without reproach—mothers who are devoted, with an ardent and self-denying affection, to the children who already constitute their family—are perfectly indifferent respecting the fœtus in utero. They seem not to realize that the being within them is indeed *animate*—that it is in verity a *human being*, body and spirit—that it is of importance—that its value is inestimable, having reference to this world and the next."

"These facts are horrible, but they are too frequent and too true. Often, very often, must all the eloquence and all the authority of the practitioner be employed; often he must, as it were, grasp the conscience of his weak and erring patient, and let her know, in language not to be misunderstood, that she is responsible to her Creator for the life of the being within her.

"After this exposition," concludes the lecturer, "and the details which have been given, and especially in view of the influence which medical science must exert on these questions, it seems hardly necessary to repeat that physicians—medical men—must be regarded as the guardians of the rights of infants. They alone can rectify public opinion; they alone can present the subject in such a

manner that legislators can exercise their powers aright in the preparation of suitable laws, that moralists and theologians can be furnished with facts to enforce the truth on this subject upon the moral sense of the community, so that not only may the crime of infanticide be abolished, but that criminal abortion be properly reprehended, and that women, in every rank and condition of life, may be made sensible of the value of the embryo and fœtus, and of the high responsibility which rests on the parents of every unborn infant."

We can indorse, to their fullest extent, the statements and sentiments comprised in the foregoing extracts from the lecture before us.

No right-minded person can view the induction of abortion, to subserve a selfish or wicked purpose, in any other light than as a crime tantamount to that of murder, demoralizing in its influence upon both sexes, as well as decidedly prejudicial to the interests and happiness of society. And they must be strangely inattentive to what is passing around them, who are not aware of the extent to which criminal abortion is practised in almost every community. Our newspapers teem with advertisements, couched in scarcely equivocal language, proffering to those who would avail themselves of them, the services of the professed abortionist.

It is, indeed, surprising with what impudence—with what an entire contempt of every moral principle and obligation, men who profess themselves to be practitioners of "the healing art," and who, perchance, are the unworthy possessors of a collegiate diploma, proclaim themselves to be ready, for a trifling compensation, to induce abortion. We were recently shown a letter containing an order for such medical books to be sent to the writer, as would prove useful to him in facilitating his practice as an abortionist; or, as he more hypocritically expressed it, would instruct him in the best and safest means for relieving females from the exposure and disgrace that an unfortunate pregnancy would necessarily entail upon them.

If it be a fact that the laws of Pennsylvania are insufficient to remedy the evil—if they consider it rather in the light of a misdemeanor than as a crime, it is high time that our Legislature should be called upon to make such enactments as will insure the conviction and adequate punishment of all who are concerned in the perpetration of this most demoralizing form of murder.

D. F. C.

ART. XVII.—*Nature in Disease, illustrated in various Discourses and Essays: to which are added Miscellaneous Writings, chiefly on Medical Subjects.* By JACOB BIGELOW, M. D. Boston, 1854: pp. 391, 12mo.

THE greater number of the essays contained in this work were published at various periods, between 1812 and the present time, and the more important of them have already been noticed in this Journal. We are glad to meet with them again in their collected form, because they are interesting in themselves, and because they are almost the only record of their author's medical opinions. His eminent position as a teacher, his large experience as a practitioner, his clearness of intellect, and his exemplary rectitude of character, give to these memorials of his professional life an absolute and exceptional value. They are peculiarly interesting to us, of a more southern sphere, as illustrating a mode of thought upon medical subjects which strongly contrasts with our own. We have more faith in the resources of our art, and cannot persuade ourselves that demonstrations of the efficacy of medicine are to be obtained by mathematical processes alone. We trust more to written tradition, and cannot easily be dissuaded from relying upon methods which bear the stamp of catholicity. It is well, perhaps, that the expansiveness of our faith should sometimes be checked by the chill breezes of northern logic. This work has such a tendency. If its style is cold, it is also clear, and occasionally coruscates with brilliant thoughts; if it is unadorned, it is charming in its simplicity; if it is somewhat rigid, it satisfies the reason by its precision and the completeness of its ideas. It is,

therefore, not an ordinary book, and it is one which we commend to all who have a relish for a pure and high tone in the discussion of medical questions.

Of the principal essay in the work, that on "Self-Limited Diseases," we might have much to say in criticism did our limits suffice; but we must restrict our remarks to a few points which appear especially to call for them. A self-limited disease is defined to be "one which receives limits from its own nature, and not from foreign influences;" and this definition agrees with the one generally given. But while in its ordinary acceptation the term is applied only to certain continued fevers, such as the eruptive fevers and typhoid fever, our author would seem to extend it to the whole nosological catalogue, except, perhaps, functional diseases of the nervous system. For instance, he remarks, "if we divide with a cutting instrument the cellular and muscular substance, we produce a self-limited disease." Surely this is equivalent to saying, that all diseases are self-limited, except those just alluded to, of the nervous system. In another place it is said: "We may suspect those complaints to be self-limited in which it is observed that the unwary and the sceptical, who neglect to resort to remedies, recover their health without them." Unless we greatly err, this passage means that all diseases which get well without treatment are 'self-limited,' and, consequently, all diseases except hydrophobia and one or two others deserve this epithet. Surely it were hardly worth while to invent a phrase which has so little in it that is distinctive.

In reviewing individual diseases, to show how slight an influence on their course and issue can be exerted by medicine, the author takes his illustrations almost exclusively from the class of continued fevers and of spasmodic affections, and intimates that art has little or no control over them. Such is not the verdict of universal experience, which asserts that the issue of these diseases is very often changed from fatal to propitious, and that the sufferings of those who endure them are greatly lessened by treatment. Are not hooping-cough, erysipelas, epilepsy, mania, &c., which he enumerates in the dark catalogue of diseases over which medical art has no certain control, are these not often, very often, cured?

"It may, perhaps, appear," says Dr. B., "that the views which have now been taken of the power of medicine in so large a class of diseases, are gloomy and discouraging, and that an unworthy tribute is paid to the labours of those physicians who have patiently studied, and arduously acted, for the benefit of humanity." Unquestionably they *are* calculated to chill all ardour in the pursuit of medicine, and make those who have consecrated themselves to it as to a crusade against suffering and death, feel that they have been the dupes of their own hearts and the victims of an imposture, an imposture which, marvellous to relate, has possessed vitality enough not only to have outlived every earthly thing contemporaneous with itself, but also to have grown to such strength and stature as insure its immortality.

The essay which presents so discouraging a view of medicine was written in 1835, and doubtless reflected in part the sceptical sentiment which had been fostered by the negative therapeutics of Broussais, and which the general attention, then turned to pathological anatomy, allowed to reach its full development. While a small portion of the profession, carried to the very extremity of scepticism, fell into the void of homœopathy, the great body retraced their steps and devoted themselves to the study of medicines and their relations to the animal economy in health and disease, so that the last fifteen years have done more to extend and define therapeutics than any equal period of medical history. That our author should have felt the latter, and salutary influence, as he also experienced the former and baleful impulse, is evident, from his essay "On the Treatment of Disease." It is true he states in his preface, that subsequent observation has not tended to shake his earlier opinions; but in the paper just referred to there is evidence enough of his occupying a more hopeful position than before. Thus he is careful to point out "many cases which are, for the most part, capable of being arrested or broken up by the interposition of remedies." And, "in other cases which cannot be thus arrested, but which, from their nature, must run a destined course, it is generally admitted that the safety of the patient may be promoted, or perhaps the duration of the case abridged,

by remedial treatment." And he goes on to indicate many occasions which illustrate the curative powers of medicines. He no longer includes in his definition of self-limited diseases all except the nervous, but establishes another category, that of the *curable*. In it we now find included those affections which "are under the control of remedies, so that they can be arrested, or abridged in duration." The powers of the antiphlogistic regimen in acute inflammatory diseases; of opiates, antispasmodics, and tonics, in spasmodic diseases; of specific remedies in gout, intermittent fever, scabies, syphilis, goitre, &c. &c., are fully dwelt upon and admitted, and it is only on approaching the class of malignant and organic diseases, that a feeling of regret is expressed on account of the finite powers of the healing art. Thus we have the satisfaction of seeing this eminent and influential teacher chasing away the despondency and discouragement inspired by his earlier instructions, and bidding us to be active and vigorous in the present, and hopeful for the future. His counsel comes to us with all the more emphasis and weight, because we perceive that it is the suggestion neither of habit nor of a dogmatizing spirit, but the conclusion of a logical mind, and that it is reached as a necessary consequence of rational experience.

The "Practical Views on Medical Education," are familiar to most of our readers in their present form, or in that which was originally presented to the American Medical Association. In this paper, it is said: "Custom in this country has generally fixed the limits of these things (lectures) at about four months. A comprehensive and judicious course, confined to the enforcing of necessary points, is far more profitable than a more discursive course to a wearied and diminishing audience." Sentiments like these, from so distinguished a source, excited surprise when they were first published. Their repetition can now only inspire regret. If it should be said that the plans of those who proposed a more extended plan of medical education have partially failed, let them enjoy the honour of having seen the necessity of rendering our collegiate instruction more extensive and more thorough, and let the profession at large bear the disgrace of being so far inferior to their guides as to desert them in their noble and self-sacrificing effort.

In the essay "On the Medical Profession," there is a very apt commentary, though certainly an unintentional one, upon this reckless haste in hurrying through the course of undergraduate studies. It is there said: "When, as it often happens in our community, narrow circumstances require that a young man should live by his own exertions, this state of things, instead of being a motive that he should crowd himself prematurely into the ranks of the profession, encumbered with debt, and bare of acquirements and of means, is rather an imperative reason that he should at once begin by resolving to devote twice the customary number of years, if necessary, to the double purpose of keeping himself in an independent position, and of placing himself at length, in point of maturity of knowledge, on a par with his more favoured competitors."

In dealing with "Quackery," the author strongly advises that it be let alone; and we most heartily subscribe to his opinion, which is thus expressed:—

"It exists in every walk and occupation of life, by the exercise of which men procure bread. The pettifogger in law, the Millerite lecturer in theology, the demagogue in politics, the system-monger in education, and the wonder-worker upon the brains and bowels of infatuated audiences—what are all these but quacks moving in their respective spheres, and fattening upon the credulity of dupes. A certain portion of mankind are so constituted that they require to be ridden by others, and if you should succeed in unhorsing a particular impostor, it is only to prepare the saddle for a fresh and more unflinching equestrian. It is not good policy to say or to write too much in regard to the pretensions of impostors. A celebrated author observes that 'many a popular error has flourished through the opposition of the learned.'[1] By throwing the gauntlet at an insignificant man, you at once raise him to the dignity of being your competitor, and acknowledge him as a 'foeman worthy of your steel.'"

The article on the "Treatment of Injuries by Fire," is interesting as showing

[1] McIntosh.

how nearly alike are the results obtained by the stimulant and by the sedative method. The author remarks, "should any one come forth as the advocate for a *negative mode* of treating burns, which should consist in letting them alone, or in leaving the process to nature; there is no doubt that in due time he would be able to collect a sufficient number of apparently satisfactory cases to answer all his purposes." This prediction, made in 1812, has been amply verified by the results of using raw cotton in the treatment of burns.

The paper which treats of the causes of "metallic tinkling" in pneumothorax, is one of the most interesting contained in the volume, and fully demonstrates that "the immediate or exciting causes of metallic tinkling is a forcible or sudden disturbance of the liquid in a vibrating cavity." This may be produced by the passage of bubbles through it, or by the agitation of the whole mass of the liquid itself.

In his paper "On the Mucuna Pruriens," the author calls in question the anthelmintic powers of this medicine; for he found by experiment that when the spiculæ were soaked in warm water for about ten minutes they lost their irritating property. He also observed that, although these spiculæ produce violent irritation when applied to the skin, they may be rubbed on the inside of the lip or tongue, without exciting any sensation beyond the ordinary mechanical stimulus.

Dr. Bigelow has collected some very interesting cases of poisoning from eating the flesh of the American partridge or Ruffed Grouse. The symptoms he mentions are vertigo, loss of sight, tinnitus aurium, and, in bad cases, loss of the power of sensation and voluntary motion. Respiration is slow; the heart and pulse are slow, feeble, and irregular; the surface cold, and the complexion pale or livid; and sometimes there is syncope. Nausea, vomiting, and abdominal pain are the symptoms affecting the digestive organs. Dr. B. rejects the popular notion of the flesh of the bird having been rendered poisonous by eating laurel berries or leaves, because these do not constitute its food, and because in the quantity taken they would be incapable of producing the effects we witness. He is disposed rather to attribute these effects either to the bird being diseased at the time of its death, to some slow chemical change, not putrefactive, which its flesh undergoes, or to the idiosyncrasy of the patients. It must be confessed that these explanations are not very satisfactory.

In the chapter on "Coffee and Tea," and in that "On the History and Use of Tobacco," we are entertained with an account of the extravagant eulogies and preposterous vituperations which all of these substances met with when first they were introduced into use. The fanatical muftis of the East are the prototypes of the reverend brethren of the West, who would drive the juice of the grape from the sanctuary, and abolish one of the holiest of religious rites rather than submit their crooked minds to be straightened on the anvil of common sense. It seems that "Coffee was introduced into Mecca, Medina, and Cairo, about the middle of the fifteenth century, and two coffee-houses were opened at Constantinople in 1554. Both at Cairo and in Turkey it had to encounter political and religious opposition; the dervises affirmed that roasted coffee was nothing but a *coal*, and that the eating of *coals* was forbidden by the laws of the prophet. So that the coffee-houses were obliged to be shut up until 'a more sensible mufti' succeeded in convincing the people that roasted coffee was not a coal, upon which they were again opened."

In another paper, the author attempts to convince his townsmen that the Cochituate water, which supplies Boston, is not made poisonous by passing through leaden pipes. The extreme excitement which disturbed the citizens of that town, as well as of New York, a few years ago, has, we believe, died away, and we hear no more of those terrible cases of lead poisoning which frightened the public from its propriety, and gave occasion to the publication of several copious and learned treatises upon the subject. We quite agree with the author that "from the present state of our knowledge, we are authorized to conclude that the insurance on a citizen of Boston, New York, Philadelphia, or London, against lead-colic, is probably worth *much less* than his insurance would be on a voyage across the Atlantic, or on a railroad for twenty miles."

A. S.

ART. XVIII.—*A Monograph on the Pathology and the Rational Treatment of Infantile Laryngo-Tracheitis or Croup.* By E. R. PEASLEE, A. M., M. D., Prof. of Anatomy, etc. From the American Medical Monthly. 8vo. pp. 32. New York, 1854.

THIS is a very able monograph. The pathological views advanced in it, when carefully and candidly examined, will, we are persuaded, be found to be, in the main, correct. They coincide, certainly, with all the facts deduced from recent observations, and divest one of the most important diseases incident to childhood of the mystery in which its true character has so generally been supposed to be involved, by indicating its analogies with other morbid conditions occurring both in the infant and the adult. Croup is an affection which has, unquestionably, nothing specific in its nature. It is simply a laryngo-tracheitis, and, as such, stands in close relationship to, or, rather, is identical with, this form of inflammation, whether it occur in the adult or infant—whether it run its course, favourably or unfavourably, with or without the production of a false membrane. Croup consists essentially in an inflammation of the mucous membrane of the larynx, extending, in most cases, into the trachea, and often into the bronchi likewise. The false membrane, when it does occur, as well as the spasmodic symptoms—the laryngismus—are both merely accessory symptoms, and altogether dependent upon and secondary to the inflamed condition of the laryngeal and tracheal mucous tissue, which alone constitutes the disease. We saw, during the last year, a number of cases of acute laryngitis, occurring chiefly in young persons, in which no false membrane was formed; and yet, in their general symptoms, these cases could not be distinguished from attacks of what has been termed genuine croup.

The source from which the false membranes are in all cases derived, and the manner in which they are developed are thus, and we believe correctly, explained by Dr. Peaslee.

"A false membrane is, in all cases, formed from the *plasma of the blood*, and which consists principally of its fibrin. It was remarked, several years ago, by Dr. Alison, that there is, in all inflammations, a *tendency* to the exudation of plasma from the vessels of the inflamed part. We maintain that in all inflammations an exudation actually does occur. It is, however, sufficient for our present purpose, that the plasma is exuded from the engorged vessels of the part, and upon the surface, in all cases of inflammation of membranes, at least. Nobody doubts this assertion in regard to serous membranes; and even the purulent discharge (often called a secretion) from an inflamed mucous membrane, is known to be derived from the plasma exuded, by a degeneration of the exudation-corpuscles into pus cells. Moreover, if an actual false membrane forms in croup upon the mucous surface, it is equally derived from the same source.

"1. The first step towards the formation of a false membrane in croup, therefore, is the exudation of plasma upon the surface of the mucous membrane, as explained, in consequence of the laryngitis. The amount of the exudation will depend upon several circumstances; of which, the state of the blood, the extent of the inflammation, and the vascularity of the part, are the most important. In membranous croup, the exudation is most abundant, and, therefore, the resulting membrane is thickest, on the surface where the mucous follicles are most abundant; and this, in the trachea, is the case with the posterior wall. The follicles are merely an *inversion* of the mucous membrane; so that here is a great amount of free surface, and a vast number of small blood-vessels inclosed within a very small space. In other parts the surface is much more smooth, the vascularity less, and thus the exudation, as well as the resulting membrane, is thinner. It has already been asserted, that it is not the secretion of these follicles that becomes organized, nor is the exudation 'albuminous.'

"2. The second step in the development of a false membrane is the organization of the exuded plasma. But it is very well known that, in the majority of instances, this never takes place, even on serous membranes; and, therefore, no false membrane is formed.

"In fact three entirely different results may ensue to the layer of plasma exuded upon the free surface of a membrane in consequence of inflammation; provided it is allowed to remain in contact with it.

"1st. It may be entirely reabsorbed. 2d. It may be converted into pus; exudation-corpuscles being formed in it, and these becoming, subsequently, degenerated into pus cells. 3d. It may become fibrillated—coagulated—and thus form a false membrane; which, subsequently, may become more highly organized, and, in case of serous membranes, even vascular, at last.

"Now, either of these results is possible, in case of inflammation of both serous and mucous membranes. In case of the latter, however, the part affected may be so situated as to be brought into contact with foreign substances—as is the case with the mouth, pharynx, and other parts of the alimentary canal—and the exudation may thus become at once detached; and in that case, neither of the three results above mentioned can ensue. Moreover, the opportunities for reabsorption on mucous membranes are very slight indeed, and often null.

"To illustrate these results in case of a serous membrane: In *pleurisy* the exudation being reabsorbed, perfect recovery takes place; if it becomes organized, false membranes and adhesions are the result; or if it is converted into pus, empyema is the result.

"Now, the circumstances favourable to the organization of the plasma are: 1st. Its perfect contact with the living tissues; 2d. Perfect rest of the part, or maintaining the required contact, and for the necessary time also; 3d. Smoothness of surface, as also subservient to the first condition.

"On the other hand, the following circumstances are unfavourable to organization: 1st. Motion of the part affected; 2d. Contact of foreign substances; 3d. Irregularity of surface. But there are certain other conditions of the exudation itself, also, which affect the result. The former is more liable to become organized, and the membrane is more perfectly formed, in proportion—1st. To the amount of fibrin contained in it; 2d. to the slowness—amount of time allowed—of the fibrillation.

"Here we should, however, remark, that all the formative processes are in much less time completed in the young than in the adult animal; and it is in accordance with this ultimate fact, that Jurine and other experimenters could artificially produce false membranes in the air-passages of young animals alone. In adult animals, the exudation would become detached before its organization was complete.

"Now, if we compare the probabilities that an exudation upon a serous membrane will become organized with those that the same result will occur upon a mucous membrane, we find the former allows—1st. Of a far more perfect contact; since it is smooth, and comparatively or entirely at rest—for example, remark the immobility of the ribs in respiration, in pleurisy. 2d. It excludes the exudation from the air and from foreign substances. 3d. Allows time for the perfection of the process of organization. 4. The fact, also, that contact of the exuded plasma with the living tissues occurs on both its surfaces at the same time—being between the two layers of the serous membrane—must not be omitted here. Nor, 5, must we fail to add, that in case of inflammations of serous membranes, the blood, and therefore, probably, the exudation also, contains a greater amount of fibrin than exists in it in inflammations of mucous membranes.

"Mucous membranes, on the other hand, are opposed to the serous, in all the respects just mentioned. 1. Having a conoidal instead of a delicate, scaly epithelium in most parts—for example, over the whole respiratory passages, and the alimentary canal, from the cardiac orifice of the stomach to the sigmoid flexure—they do not allow of so perfect a contact of the plasma with the living tissue of the membrane itself. 2. They afford contact only on one side of the plasma. 3. In most parts, the plasma is exposed to the air and the con-

tact of foreign substances. 4. There is more motion of the part. 5. All these circumstances afford less time for the organization to occur; so that it may fail of being completed if commenced. 6. And, finally, the plasma itself is less organizable than in the case of inflammation of serous membranes.

"We have no difficulty, then, in accounting for the fact that false membranes occur very much more frequently on serous than on mucous membranes. Nor is there any in accounting for the fact that, in certain cases, they are formed in the latter also, as well as in the former.

"False membranes have been known to form in cases of inflammation of the rectum, the vagina, the uterus, the nasal passages, the pharynx, larynx, trachea and bronchial tubes; and in all these parts certain peculiarities exist, more favourable to the organization of the plasma than the mucous membrane in other parts presents. The rectum and vagina have a scaly epithelium, and are comparatively at rest—the rectum, even, being void of fæces and of motion, except during the act of defecation. The uterus, also, allows of still greater rest of the exuded plasma, though it has a conoidal epithelium. The whole extent of the air-passages is provided with a conoidal and a ciliated epithelium; but it has no foreign body in contact with it, except the air; and in this respect, and in point of motion of the surface, has the advantage of the alimentary canal above the rectum; where false membranes do not become organized. In children, however, there is less motion still; and they also requiring less time, we should expect organization to occur more frequently in them than in adults."

Instead of the foregoing long extract from the monograph of Dr. Peaslee, we might have contented ourselves by giving, in a brief synopsis, the leading views embraced in it. But, as the presence of a false membrane in cases of croup appears to have been the chief stumbling-block in the way of arriving at a correct understanding of the true pathology of the disease—while upon it has been attempted to be based the evidence of the specific character of the malady, we were desirous of presenting at length, and in his own words, the author's exposition of the source and mode of development of false membranes as a result, under certain circumstances, of simple inflammation of the tissues upon which such membranes occur.

Returning to the subject more immediately under consideration—the pathology of croup—Dr. Peaslee remarks:—

"In cases of actual laryngitis with false membrane, or membranous croup in children, the exuded plasma becomes to some extent organized, *fibrillated* at least. But can we suppose that this result occurs in every case of laryngitis in the early years of life? If the plasma is often absorbed, and also frequently converted into pus, in case of inflammation of a serous membrane, as in pleurisy, can we doubt that either or both these results often occur in cases of inflammation of any and of all mucous membranes? And to say that croup is a peculiar or *specific* inflammation, in order to account for the formation of the false membrane, would, for the sake of consistency, compel us also to invoke a *special* inflammation in those cases of pleurisy in which adhesions occur, while a *common* inflammation will answer if recovery occurs by absorption, or if empyema ensues. Our ideas, then, of the pathology of croup, so far as the answer to the first inquiry has established them, are these:—

"1st. An inflammation of the larynx, extending into the trachea, occurs; offering in its essential nature nothing different from any other case of inflammation of the same parts, either in the infant or the adult. It is generally preceded, in both infants and adults, by congestion and irritation, and therefore by catarrh.

"2d. An exudation of plasma occurs on the inflamed surface, as in the adult, this being most abundant in the trachea on the posterior wall, for reasons already given.

"3d. This exudation may be disposed of, in at least two ways, provided it is not at once removed, as it generally is, in adults but not in infants, by coughing; reabsorption probably very seldom occurring in this disease, though it is not impossible. *a.* It may become degenerated into pus—purulent matter—and thus, of course, at once be detached, which is the most common result.

b. It may become organized into a false membrane. This is more probable if the blood is rich in fibrin, as in a plethoric child; if there is but little cough, an adult generally expelling it thus; and if time is allowed for its development, less being required in the child than in the adult.

"4th. Croup is, therefore, merely a laryngo-tracheitis in infants and children, and offers nothing essentially different from the same inflammation in adults. The exudation in case of adults is, however, usually at once ejected by coughing, or in the form of purulent matter; while the liability to its organization in infants is greater; though, after all, a comparatively rare result, considering the whole number of cases, for the reasons before mentioned.

"5th. Practically, therefore, as well as pathologically, we cannot say with Bouchat, '*without a false membrane, croup does not exist.*' This membrane never exists till the inflammation—the essential element of the disease, as we believe—has preceded, and has produced the exudation of plasma, as before shown. No sooner does the catarrhal *irritation* merge into inflammation, than the plastic lymph is thrown out; and this *inflammation and its accompanying exudation* are the elements always present in croup.

"We, therefore, need not, for any practical purpose, admit an 'inflammatory and membranous' croup, as some writers have done, any more than we should make the same distinction in regard to pleuritis or peritonitis. All *croup is inflammatory*, at any rate; and a few cases are also accompanied by the formation of a false membrane. But the latter should not affect the treatment of the disease as an inflammation, but merely from its mechanical effects, and cannot be predicated in any case till it is actually seen; and this is not possible in most cases in which it is developed at the very onset of the disease. Finally, we would drop the term croup entirely, and use the term laryngo-tracheitis instead. In a work on the diseases of children, we would call particular attention to the fact that a false membrane is formed in about one-sixth of all the cases of this disease; while in adults, this is of very rare occurrence. But we would not make an accident the distinguishing feature of this disease, any more than we do in the case of others, nor allow it to enter into either our name or our definition of it.

"As in all other inflammations, so in this; the distinction of 'sthenic' and 'asthenic' is important, both in a pathological and a therapeutical point of view. So far, also, as laryngismus enters into any particular case—and it does into all cases of true laryngitis to some extent—the case is, of course, *spasmodic;* but this term must not be applied to the exclusion of the idea of inflammation. *Genuine spasmodic* croup, we have already seen, is a mere laryngismus. There is more or less spasm in all cases of bronchitis, and still more in whooping-cough; in the latter case in the larynx also; so that infantile laryngitis does not present any peculiarity in this respect."

In regard to the question: Does laryngitis and tracheitis, with false membrane, occur as well in the adult, as in early life? Dr. Peaslee says:—

"We do not hesitate to answer this question in the affirmative, having seen such membranes ourselves, and having demonstrated their fibrillated structure under the microscope. Such cases are comparatively rare, for the reasons already assigned; and therefore occur more frequently in females who are not accustomed, or, as in some instances, not able, to expectorate at all. I received and still retain such a membrane, expelled in a tubular form by an adult female patient of Dr. G. W. Garland, now of Lawrence, Mass. Even 'membranous croup,' then, if we retain the term, occurs, though rarely, in adults, it being, at all ages, essentially the same disease so far as its pathology is concerned."

To the question: Does the danger to the patient, in infantile laryngo-tracheitis with false membrane, result almost entirely from the presence of the membrane, as seems to be generally admitted, or is the inflammation alone often sufficient to produce death? Dr. Peaslee answers, that—

"Surely no reason can be assigned why a disease so fatal to adults as laryngitis and tracheitis should not be equally so in early life. And yet, since the disease of the 'Father of our country' to the present time, adults have died without the formation of a false membrane, in almost all instances, so far as can be ascertained.

"The fact cannot be overlooked, that an inflammation of the larynx is a serious and a dangerous matter, at any time of life, as a *mere inflammation*, aside from the final disposal of the exuded plasma. The treatment is, therefore, to be directed primarily to the disease as a mere inflammation. Of course, the formation of the membrane brings a new danger to the patient, since it partially, and, if it extends to the bronchial divisions in the lungs, in some parts completely, closes the air-passages, and thus produces a gradual asphyxia. Occurring, as it does, also, after the patient is reduced by the previous inflammation, its dangerous effects are increased. But we must, however, bear in mind the fact that the membrane is not organized till the *inflammation has subsided* in the part. The inflamed vessels relieve themselves by the exudation, and thus the inflammatory process is arrested. The treatment, therefore, to subdue inflammation, so proper at first, is *not* proper after the false membrane is formed in the larynx and trachea. We have here a *consequence* of inflammation to treat, in a part which the inflammation itself has left. The thicker the membrane, the greater the danger and amount of asphyxia; but the small trachea of an infant being closed in a greater ratio by it than the larger tube of the adult, the danger is comparatively slight in the latter from its formation. It is seldom more than one and a half lines thick at the thickest part, in the child; and usually one-half to one only.

"The progress of laryngo-tracheitis," Dr. P. remarks, "is invariably from above downwards; the observations of the best pathologists have established this point. Generally, or at least very frequently, a catarrh commences in the nasal passages, and extends backwards into the pharynx; then descending into the larynx, continues such for a time, or at once merges into an inflammation. Often the pharynx becomes inflamed, and a false membrane appears upon it before the inflammation descends into the larynx and trachea. In some cases the larynx may be the part first affected; but if so, such cases are to be regarded as exceptional, and the inflammation never extends upwards, but always downwards from that organ. The existence, therefore, of a catarrh for two or three days, and especially of a false membrane on the pharynx, before the peculiar ringing cough of infantile laryngitis occurs, are more important elements of diagnosis in this disease, though their absence is not *demonstrative* of its non-existence. It will be recollected, therefore, that we said the exudation of plasma relieves the inflammation of *the part*, though at the same time it may be extending downwards to other parts lower in the air-passages.

"In infantile laryngitis it has been stated that its progress is downwards into the trachea. In some cases, and not unfrequently, it extends into the bronchi, and even into their subdivisions into the lungs. The exudation is sometimes so copious in the smaller tubes in the lungs, that they become filled with a solid cylinder of plasma, instead of being lined with a false membrane. The same often occurs in cases of bronchitis in adults. We can have no warrant that this result will not occur in any case where a false membrane has been formed; nor that convalescence will commence at once, though large portions of it have been expelled from the trachea. Nor can we expect the convalescence to progress rapidly as soon as the laryngismus and ringing cough have ceased, since the bronchitis may still remain for a few days, or may even prove fatal at last.

"Some seem to think that, if the false membrane is removed from the larynx and trachea, all danger is over. Nothing, however, is gained by merely passing the air through the *air-passages;* it must enter the *air-cells* before the blood is aërated thereby. Whether, therefore, the trachea is diminished to one-half its capacity by a false membrane; or one-half of the smallest bronchial tubes are completely closed by solid cylinders of plasma, will not make the least difference so far as the production of asphyxia is concerned.

"The false membrane is often formed in a very short time after the exudation is poured out; but it is an important fact that a false membrane formed on a mucous surface never becomes vascular, and therefore permanent, as it may on a serous membrane; but it *soon becomes detached, and spontaneously falls off.* This is well-known to be the case in vaginitis, and the endo-metritis which sometimes accompanies dysmenorrhœa; and the disease now under consideration

is not at all different from these in this respect. Only a fibrillation existing in the false membrane, and, therefore, no vascular connection between it and the subjacent surface, it soon loses its slight vitality, and is cast off accordingly.

"The time elapsing before the spontaneous detachment of a false membrane varies in different cases, but seldom exceeds five or six days after its formation. But this result will surely occur if the patient's strength continues, and his life is sufficiently prolonged to afford the necessary time.

"It has been seen that laryngo-tracheitis occurs in the adult as well as in the infant; but is not the 'Diphtherite' of M. Bretonneau a different disease?

"We see no necessity," says Dr. P., "for any such admission. In diphtherite the false membrane almost always covers the pharynx and tonsils, and often extends into the posterior nares, covering both surfaces of the velum, and then descends into the larynx and trachea. The same often occurs also in connection with scarlatina and rubeola. But all this also often occurs in the young child, and therefore the terms 'Diphtherite,' or 'Croup of Adults,' we consider objectionable, as calculated to foster the idea of a radical difference as to the nature of inflammation of the larynx and trachea in the young child and the adult, while they are pathologically the same."

Although, from a careful perusal of the foregoing extracts from the monograph before us—for the copiousness of which we hardly think we need apologize, considering the importance of the subject to which they refer—our readers will acquire a very correct idea of the views of Dr. Peaslee in reference to the pathology of croup; nevertheless, the following recapitulation of the more prominent of those views will not, we are persuaded, be unacceptable:—

"1. Croup is always a laryngo-tracheitis, or inflammation, not of a specific kind, of the mucous membrane of the larynx, extending thence into the trachea also at least.

"2. Like other inflammations, it may be either sthenic or asthenic.

"3. Its progress is always downwards in the air-passages, whether it commences in the larynx, or, as is more frequently the case, higher up.

"4. It is always attended by an exudation of plasma upon the surface of the inflamed membrane.

"5. The inflammation subsides in the part on the occurrence of exudation.

"6. The plasma *remains* on the surface—it being usually detached and removed in case of the adult by coughing—it may, *a.* Become reabsorbed, which is a rare result; or, *b.* Become converted into purulent matter, and then excreted; or, *c.* Become organized into a false membrane. This last result is far more common in early life than in adults, though then occurring only in about one-sixth of all the cases. Consequently it can never be predicted, at the commencement of the disease, whether a false membrane will, or will not, be formed.

"7. If a false membrane is formed, it will become spontaneously detached, if the patient's life is sufficiently prolonged. Its removal, however, in whatever way effected, does not *insure* recovery.

"8. Laryngismus is associated, to some extent, with true laryngo-tracheitis, as a matter of course.

"9. Catarrh generally precedes laryngo-tracheitis. But the catarrhal discharge, *so called*, during the more advanced periods of this disease, is the purulent fluid formed by the degeneration of the exuded plasma.

"10. Diphtherite, or the 'croup of adults,' differs not *pathologically* from infantile laryngo-tracheitis. Generally, however, it is an asthenic form of laryngo-tracheitis, since it usually attacks persons already debilitated by other diseases."

It is impossible for us to follow the author of the monograph before us in his discussion of the rational treatment of infantile laryngo-tracheitis directly based, as he supposes, upon a correct pathology of the disease. The treatment he recommends, and, in general, the means upon which he would depend for carrying out his general indications, have our hearty approval. In some few particulars—in the comparative value of this or that remedy—and the stage of the disease to which it is best adapted—we may differ from him; but, in its general outlines, his therapeutical directions are sound and judicious. A general abstract of them would be of little value—to be properly appreciated they must be studied

in extenso, in connection with the comments with which the author has accompanied them.

We can only give here the following tabular view of the treatment recommended by Dr. Peaslee for the two forms of the disease, as drawn up by himself:—

INDICATIONS.	STHENIC LARYNGO-TRACHEITIS.	ASTHENIC LARNYGO-TRACHEITIS.
I. To arrest the inflammatory process, and control the laryngismus.	An emetic, followed by a full dose of calomel, if required.	Do. do.; or pill. hydrargyri, instead of calomel.
	Leeches to the throat, or over the sternum; followed by cold applied continuously.	Leeches doubtful. Cold continuously applied.
	Tartrate of antimony; sedative doses, cautiously administered.	Nitrate of potassa.
	Internal application of solution of nitrate of silver.	Do. do.
	Inhalation of aqueous and narcotic vapours.	Do. do.
	Opiates, with caution. Hydrocyanic acid, do. do.	Do. do.
II. To prevent organization of the exuded plasma.	Sponge-probang and caustic solution.	Do. do.
	Expectorants.	Stimulating expectorants.
	Calomel, in small doses.	Turpeth mineral.
	Alkalies.	Do. do.
	Counter-irritation.	Do. do.
	Tracheotomy?	
III. For removing the false membrane when formed.	Continue caustic solution.	Do. do.
	Do. expectorants.	Do. do.
	Do. counter-irritation.	Do. do.
	Do. alkalies.	Stimulant alkalies.
	No emetics, or calomel.	Do. do.
	Tracheotomy, *seasonably*, if at all.	Do. do.
IV. To sustain the strength.	Milk-porridge, arrowroot, broth, &c.	Do. do.; also wine whey, milk punch, &c.
	Tonics, if required.	Tonics and stimulants.

The entire monograph we would very strongly recommend to the attention of the medical public as being among the very best that have appeared in reference to the disease of which it treats.

D. F. C.

Art. XIX.—*A Practical Treatise on the Diseases Peculiar to Women, Illustrated by cases derived from Hospital and Private Practice.* By Samuel Ashwell, M. D., Member of the Royal College of Physicians, London; and late Obstetric Physician and Lecturer to Guy's Hospital. Third American, from the Third and Revised London Edition. 8vo. pp. 528. Philadelphia, 1855. Blanchard and Lea.

The treatise of Dr. Ashwell has been already assigned a high rank among the leading authorities on the diseases of the female; and deservedly so. Although less full and comprehensive than one or two of the modern works on the subject by distinguished Continental writers—it being confined mainly to a consideration of the affections incident to the non-pregnant state—nevertheless, the clear, definite, common sense, and practical manner in which the subjects embraced in it are treated—more weight being accorded to the results of positive and repeated observations than to the deductions from specious hypotheses, and to *a priori* reasonings in favour of any special system of medi-

cation—renders the treatise of Dr. Ashwell a valuable and trustworthy one for the use of the student and young practitioner.

The account given by Dr. Ashwell of the pathology and therapeutics of the maladies of the female sex, of which he treats, are based chiefly on the results of his own clinical observations, made under the most favourable opportunities for the study of these diseases; at the same time, however, he has not been neglectful of the labours of others in the same field; in fact, nothing of importance appears to have been overlooked by him "in the progress of that department of medical science to which the work is devoted, that could either assist the practitioner or inform the student."

Of the various diseases treated of by Dr. Ashwell, he has narrated in more or less detail, numerous cases, in order that the symptoms of these "may show whether the histories of the diseases are accurately given," and "from their successful or unfavourable issue, the danger of the malady and the worth of the treatment may be demonstrated."

We agree with the author that well delineated cases of the diseases treated of "add also greatly to the interest of an elementary work, as they relieve the dulness from which a constant repetition of principles, without such portraits, is nearly inseparable." D. F. C.

ART. XX.—*Anniversary Discourse before the New York Academy of Medicine. Delivered in Clinton Hall, November* 22, 1854. By JOHN H. GRISCOM, M. D. Published by order of the Academy. 8vo. pp. 58. New York, 1855.

WE have read with uncommon interest this address of Dr. Griscom, and could wish that it were possible to place a copy of it in the hands of every citizen of these United States, and induce him to reflect carefully and deliberately upon the important themes it discusses—themes in which it is not the physician alone who is interested, but which involve the comfort, and health, and life of each individual of the entire community.

The relation between the people and the science of medicine is the subject of Dr. G.'s discourse, and with a manly vigour of style, great plainness of speech, and clearness of demonstration, he shows the important and continued benefits which legitimate medicine, administered by a well-educated, skilful and devoted body of physicians, confers upon society, as well by its therapeutic or curative means as by its prophylactic or preventive powers. And this, with small return on the part of the public to its professors, in the form of gratitude or pecuniary compensation, or even of a just respect for their proper rights and privileges.

The immense amount of gratuitous labour performed by the medical profession, at the sacrifice of the time and comfort, and at the imminent risk of the health, and even life of its members; of which labour the community at large reap the reward in the promotion of their best interest, as well as in merely a pecuniary point of view, is very forcibly depicted by Dr. Griscom.

"Can there be named," he remarks, after pointing to facts which cannot be disputed; "can there be named any profession, high or low, feeble or strong in numbers, that gives the proportion of a thousandth part of the amount of service to charity, or public weal, that is given by the medical profession annually, without fee or reward? The glittering enticements of wealth, apparently so easily attainable in the counting-house; the allurements of popularity held forth from the hustings or the forum; the ambition to command the applause of listening senates, or the bubble reputation at the cannon's mouth; these are all suffered to pass as the idle wind, by him who finds a luxury in doing good, be it in the gilded and satin-furnished chamber of the millionaire, or the stripped and impoverished cellar or attic of the child of want and misery.

"But the response is upon the lip of the layman, 'you receive an equivalent in enlarged experience, and the hospital and dispensary appointment gives a

credit which is a passport to private success.' Without stopping to criticize this assertion, which is so amenable to criticism by those who best understand the subject, let me simply say that the same amount of experience would be afforded, and an equal influence upon private success be exerted, if the incumbents of these appointments were to receive a pecuniary remuneration for their services; in other words, if the members of the profession were to-morrow unanimously to decline these incumbencies, and the authorities were compelled to pay a pecuniary equivalent for the required medical and surgical aid, while, in either case, the experience alluded to is more, far more, for the advantage of the public than for that of the profession; and in the establishment of hospitals, dispensaries, and asylums, the public receives back, in this very experience, all it pays for their support, with decupled interest. In whatever light it may be viewed, therefore, the rendition of these services is simply the *contribution of the medical profession to the support of public charity to the full amount mentioned*" (nearly one million annually in New York alone); "it is so much saved to the tax-payers.

"I would not be understood as recommending, or even suggesting, any alteration in the time-ennobled and heaven-born spirit which actuates our profession in this matter; and I am fearful that, even so far as I have gone, I have not only trespassed upon the sacred injunction of Christian charity: 'Let not thy left hand know what thy right hand doeth,' but that I have also violated one of the most honored rules of medical ethics. If I have erred herein, the blame is exclusively my own. I have but sought to place the profession of my choice '*rectus in curiâ*' in the great assembly of the people, which contains so many who affect to believe in its hard-hearted and exacting disposition, and who flippantly charge it with inhumanity, when it seeks the wherewithal to obtain some of the creature rest and comfort, which all value so highly, but which the physician's very duties, in a great degree, deny to him."

Dr. Griscom next points out the disregard shown by the public to medical science and its legitimate professors, by the liberal encouragement bestowed upon every form and phase of charlatanry, and refers to the baneful effect this produces upon the progress of science and the improvement of medicine.

"I have before," he remarks, "adverted to the fact, that it is necessarily to the scientifically educated members of the medical profession, and to them only, that the world can look for progress in medical and surgical art. To this duty the *attention*, and no inconsiderable portion of the *means* of the profession, are continually addressed. Its journals are supported by itself alone; except in one of these United States (Michigan), medical schools, unlike our ward schools and Free Academy, which are sustained by the public, derive their support almost wholly from the profession. The expenses incurred by the numerous State, county, and private medical societies, are defrayed, to the last cent, without aid from that public whose interests are deeply involved in their continued and energetic action; while the time and talent expended in gratuitous services in medical charities, as has already been stated, redound greatly to the advantage of the public."

"Under this state of facts, it appears certainly due to the profession that it should receive from the public, whose interest it serves, all the aid and support which is legitimately derivable from the practice of the healing art. To put money into the pockets of an empiric, although his treatment may occasionally *hit right*, is to support a class of men devoted exclusively to private gain, without the least public spirit in their calling, who have never yet given the wheel of medical science a single revolution forward, and from their ignorance could not, if they would.

"This position I am ready to abandon when there shall be shown one new idea in chemistry, a single ray of light from the profound depths of physiology, one anatomical discovery, or a reliable idea before unknown in pathology or therapeutics, that has ever emanated from the head of any medical charlatan that ever lived. In fact, a truly scientific man cannot be a quack.

"The loss to the medical profession, therefore, by the support of charlatanry, though it has been enormous, is far surpassed by the loss sustained by *medical*

science, and consequently by mankind, for whose use all sciences are studied and improved."

Three means are indicated by Dr. G. for promoting the prophylactic or preventive powers of medical science, and the adoption of the means it indicates for the removal of the causes of disease.

The first of these is, *the universal introduction of Human Mechanism and Physiology, as a subject of study in schools, especially the public schools.* The second is, *the establishment of a voluntary association*, composed jointly of laymen and physicians, whose objects of inquiry and action shall comprise the entire field of the preservation of public and private health, in all its varied and multitudinous relations.

"I believe," says Dr. G., "that on no subject would sound and authoritative information be sought for with more avidity. By no other means could more practical general good be bestowed; by none could more lives be saved and prolonged than by such a society, energetically conducted.

"The argument for the formation of such an association exists in the fact that there are numbers of able men who have devoted much time to the study of Hygiene, who have done much, and are prepared to do more, to enlighten the public mind, but whose labours are rendered comparatively useless for want of union and concentration. Their individual efforts, dispersed through ephemeral publications, are productive of but feeble effects, when, by combination and a common channel, through which to pour the results of their investigations and studies, the good would be effected for which they now vainly pray and labour.

"Such a society would study out the true method of city sewerage, generally heretofore committed to most unscientific hands, and from which, valuable as it is when rightly conducted, there is great reason to apprehend serious evils. It would infuse into the public mind, into our legislative bodies, and our magistracy, a more intelligent regard for the proper construction of private and public edifices of every description; it would point out where and why certain diseases prevail, and how they might be avoided; it would obtain an influence in the proper regulation of health measures and the enactment of sanitary laws which would be of incalculable benefit to the commercial prosperity of this and other cities, and the happiness and comfort of their every inhabitant. It could do much by exerting its influence in the right direction to secure that great desideratum, a sanitary survey of the State, and obtain through the census a vast amount of important medical statistics, now almost wholly lost; it could materially aid in perfecting and establishing a complete system of registration of births, deaths, and marriages, now entirely neglected for want of proper intelligence and energy in the right quarter. In a word, it would take under its keeping and guidance the sanitary interests of the whole people, in all their aspects and relations, and, as a medium of communication between the people and the profession, would exert a powerful influence for the good of both."

The third and last suggestion of Dr. G. is the establishment of a *health police.*

"Since," he observes, "it is manifest that the aid of law must be invoked for the suppression and prevention of disease-producing circumstances, it would appear at first blush that, for an intelligent and satisfactory performance of this important and very nice duty, some degree of medical knowledge were requisite. In many instances it requires no little scientific acumen to determine what really constitutes a nuisance. Even for the mere purposes of *inspection* of the domicils, workshops, manufactories, and places of congregation of every kind, it were but common sense to acknowledge the necessity of a certain degree of familiarity with the laws of health and the causes of disease, to say nothing of the many other more delicate and responsible duties implied in the name of a preventive sanitary police.

"No argument can be needed to enforce so self-evident a proposition as that a *health* officer should possess a medical education; and yet, will it be believed, that the commercial metropolis of the western world, with a population rapidly approaching a million, has a sanitary police which would not only fail to be recognized as such by its name, but among the twenty-nine persons constituting

it, there is—and that only by chance—but one medical man, and he with no concern in its practical prophylactic duties. With this exception, from head to foot, not a member of it probably would be able to distinguish incipient smallpox from mosquito bites; and, if told it were the former, would most probably give it a wide berth, rather than personally attack so formidable an adversary."

"It is not enough that the sexton's weekly returns of the causes of death are summed up and given to us in annual tables by the yard in length, which a little arithmetic will accomplish. We need to know the *causes of the diseases;* where they are, what they spring from, and how to avoid them; information which can only be obtained and properly disseminated by men of good medical education, and possessing the proper taste and energy for such work."

There is great truth in the concluding words of the discourse before us. Would that the public could be induced to give ear unto them, and profit by them.

The votaries of medical science, whatever impediment may rise up in their path, will, Dr. G. remarks—

"Still move forward on the great errand of human health and life, enlarging its borders, and strengthening its stakes; feeling that upon their energy in the prosecution of improvement, and the enlightenment of the public, depend in a great measure the health, the strength, the longevity, the mental, moral, and physical condition of communities, as well as of individuals, and upon them depend the welfare of the State, and even the stability of the government itself. A republic of imbeciles, of dwarfs, either of body or spirit, of a people wasted by disease and deprived of the natural provocatives of health, is as impracticable as a heaven of profligates and demons; and without sound bodies, sound minds are impossible."

D. F. C.

ART. XXI.—*Outlines of Chemical Analysis prepared for the Chemical Laboratory at Giessen.* By HEINRICH WILE, Professor of Experimental Chemistry in the University of Giessen. Translated from the third German edition, by DANIEL BREED, M. D., late of the Laboratory of Liebig, Löwig, and Wile, and LEWIS H. STEINER, M. D., Professor of Chemistry in the National Medical College. Boston and Cambridge, James Munroe & Co., 1855. 8vo. pp. 298.

THE Laboratory of Giessen has so long been the most prominent resort for the acquisition of a practical knowledge of chemistry, and its conductors have acquired so firm a reputation for successful teaching, that any work in relation to chemical analysis emanating from this source, and founded on the course pursued there, must necessarily command attention. Heinrich Wile, under whose supervision it has some time since passed, has, in this third edition of his Outlines, given the results of his increased experience, and added to the examples in quantitative analysis. The first edition was soon translated by Professor Hoffman, and adopted in British and American laboratories as a safe guide, but the advance of analytic chemistry has rendered improved methods of research necessary, which are here fully supplied. The most marked feature in the two late editions is the increase in the number of tables, by which the student is to be guided in his researches, and is more easily led to his results and determinations. The translators have given a very satisfactory transcript of the original, and have, no doubt, adhered with strict accuracy to the ideas of the author, having previously been personally the recipients of the instruction here conveyed in the printed form. They have also given an introductory chapter on apparatus and manipulation, very judiciously adapting the work to the wants of those to whom this part is not familiar.

The mechanical execution of the book is good, and the type clear and of good size, not at all calculated to fatigue the eye; even in the tables, where restricted space necessarily produces a reduction, and which, from their constant use as references, should be entirely free from any drawback of this kind.

R. B.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Experiments on the Innervation of the Heart.* By Prof. Rudolph Wagner, of Gottingen.—I have been engaged in making experimental investigations on the innervation of the heart, during several winters, in the Physiological Institute here, especially in cold-blooded animals. The present method of experimenting on warm-blooded animals, by means of opening the thorax and conducting artificial respiration, or by means of the kymographion or auscultation, are open to objections on account of the inconveniences attending them, and on account of the injuries done to the animal, which occasion great disturbances in the action of the organ. While spending a short time last autumn in Breslau, where I found prevailing a very lively interest in physiological and pathological investigations, Dr. Kliddedorpf communicated to me some experiments he had made on apparent death, in which he had introduced into the heart a slender needle, the movements of which gave indications of the activity of this central organ of the circulation for some time after the cessation of respiration and of the pulse. I immediately resolved upon acupuncture as a means of a new series of experiments on the innervation of the heart—a means which, as far as I know, has never yet been applied to this subject. The very first experiment showed that the vibrations of a needle introduced into the heart are not only extremely favourable for determining the number of contractions, but also for ascertaining their intensity, and other phenomena, even to accurate measurement. Assisted by a number of my pupils, I have been occupied since last November in carrying on a great number of experiments on the innervation of the heart, and I should have postponed for the present the publication of the results obtained, had I not lately learned that experiments upon the same subject are being made at other points.[1] I therefore presume to hope that the improved method of experimenting in use here may be made use of and perfected by others.

The animals employed here have been generally rabbits, and sometimes cats and dogs. The needles used at first were of steel, about ninety millimetres in length, and with a head about six millimetres in thickness, which can be made hollow, that the weight may not be too great. The situation and anatomical relation of the heart of the animal to be experimented on having been accurately learned, the needle is to be introduced through the parietes of the chest into the left ventricle near the apex. The vibrations of the needle are the expression of the beats of the heart. As the needle has its point of support in the skin and intercostal muscles, it must be somewhat influenced by the

[1] For instance in Halle, by Heidenhain Fechner's Centralblatt für Naturwissenschaften und Anthropologie, 1854, No. 7. Also in Zurich by Ludwig and Weinmann.

movements of respiration; but this does not generally occur to a degree worthy of consideration. The number of vibrations gives the number of contractions or beats of the heart, the extent of the amplitudes (the point of support remaining the same), the intensity of the contractions, the so-called impulse. The head of the needle, as it rises and falls, can now be allowed to strike on a sonorous body, such as an inverted wineglass fixed to the movable arm of a retort-stand, so as to be placed in any situation. A few trials will be sufficient to convince any one that the innervation of these strokes is far easier and more certainly performed than counting the pulse of a bound and perhaps struggling animal. Besides, in this way the enumeration can be carried on by a great number of persons at once—a point of some importance if the number of contractions per minute be considerable, as for instance in the rabbit, where they amount to two hundred or more when the animal is at rest, and are much increased both in number and velocity when both *vagi* are divided, as shown by the increased force of the blow, and the greater amplitudes of the needle. This simple instrument is, therefore, sufficient for a great number of experiments on the innervation of the heart. The vibrations of the needle take place with great uniformity—even on its introduction, but little irregularity is occasioned—the number of contractions not being increased more than from two to three per cent., and this only lasting a short time. After the removal of the needle, the animals appear quite well. For experiments on dogs, sometimes even for rabbits, a trocar, not exceeding one millimetre in thickness may be made use of, without causing permanent injury to the animal. If it is wished to observe the contractions of the auricle, another and finer needle must be introduced, preferably into the right auricle.[1] If another glass is now placed for this needle to strike on, a pleasant play of bells is produced by the rapidly following blows, in which the ventricular needle leads. In this manner the systole of the auricles and ventricles can be made audible and visible to hundreds of persons at once, and if now another needle is introduced through the parietes of the thorax, above the diaphragm, and another glass placed for it, the number and strength of the respirations are made evident at the same time.[2]

If it is merely wished to observe the number and impulse of the contractions during irritation of the nerves which influence the heart's action, a single needle is sufficient introduced through the intercostal space, immediately above the left ventricle, as more simple pendulum movements are obtained here than by its introduction nearer the apex, where the well-known burning movement will give it more rotation.

It is evident that this simple apparatus can be used for accurate measurements. The movements of the needle are sufficient in extent and power to set in motion delicate levers, which can be used in the usual manner for the delineation of curves. In this manner, by means of the abscissa, the very slight intervals of time which here occur, and by means of the amplitudes, the strength of the contractions, which we name the intensity of the heart-movement, can be graphically delineated. Thus far, I have made but few experiments in this manner, as practical difficulties are to be overcome. For a great number of experiments, which I have already made, the simple needle is sufficient, and on account of its simplicity and the ease attending its use, does it appear especially worthy of recommendation.

The first question I applied myself to answer was: From what parts of the nervous system can the heart be affected in its movements, and what is the character of the symptoms produced?

[1] The introduction of the needle into the auricle has its difficulties, which must be overcome by dissection. Extravasation is frequently produced by piercing the vena cava: this may also occur by wounding a branch of the coronary vein on introducing the needle into the ventricle.

[2] The degree of interest maintained in his class by this celebrated physiologist is exceeding great. The combination with lectures of experiments, in which all the students take part, is in the highest degree interesting and instructive; and this mode of teaching physiology, so general in the German schools, presents a marked contrast with that prevalent among us.—[*Translator.*]

It is already a well-known fact that an interrupted electrical current, as produced by the common induction-apparatus, when applied to the roots and branches of the *vagi* of all the vertebrata, produces retardation of the movements of the heart, considerable pauses between the beats, and even an entire cessation in the diastole, during which only slight contractions of single bundles of fibres are observed. With a powerful stream, the inclosure of only a single *vagus* in the circuit is sufficient, even in mammalia, to produce cessation of the heart's action. But, nevertheless, I believe I have discovered that this cessation is never so complete and enduring when the poles are applied to the *vagi*, as when they are applied to the *medulla oblongata*. This is particularly the case in birds, the cessation being far more perfect when the medulla is included. In frogs and fishes, a total cessation of several minutes' duration can be caused, after previous removal of the encephalon had increased them.[1] I concluded, from this circumstance, that there may be other fibres proceeding from the medulla, not included in the vagi, which exert a retarding influence upon the heart's action. It was necessary, therefore, to examine the cervical portion of the *sympathetic*. Weber supposes that the sympathetic exerts an influence opposite to that of the vagus, but he has not succeeded in proving this by experiments.[2] More frequently, the ground is now taken, in physiological works, that increased contractions of the heart follow irritation of the cervical portion of the sympathetic, a doctrine which Liebig has lately opposed, without, however, adducing anything definite in regard to the influence which is really exerted by the sympathetic upon the movements of the heart.[3] I found by a great number of experiments made on rabbits, with all precautions, and with various forms of apparatus, that when both sympathici were acted on there was constant diminution in the number of beats of the heart, nevertheless, not in the same degree as in the case of the vagi. The decrease amounts to one-fourth, one-third, or even one-half of the normal number, depending for the greater part on the power of the current. Generally the decrease is in a constant proportion, as the following experiments on a rabbit show:—

	Beats of the Heart in fifteen seconds.		Beats of the Heart in fifteen seconds.
Both sympathici included	17	After breaking the circuit	43
" " "	16	" " "	44
" " "	18	" " "	36
" " "	18	" " "	35

Constant increase of the beats of the heart is occasioned: 1. By including the sympathici in a constant stream. 2. By tying and dividing the cervical portion of the sympathetic. I prefer ligation and division to the use of the electrical current as producing no irritation of neighbouring nerves nor other disturbing symptoms. Under favourable circumstances, the following or similar results will be found:—

Experiment on a full-grown Male Rabbit; number of beats of Heart in fifteen seconds; five enumerations.

At rest.	After exposure of nerves.	After tying sympathici.	After dividing sympathici.[4]	After tying right vagus.
61	59	64	67	77
60	61	67	68	76
62	60	68	68	75
60	60	68	67	78
60	62	68	64	79

[1] For example, in frogs from six to ten beats.

[2] Handworterbuch der Physiologie, vol. iii. sec. ii. p. 47.

[3] Lehrbuch der Physiologie, vol. i. p. 178: "As regards the rabbit, this statement is entirely wrong, as I have satisfied myself by thorough investigations conducted by Weinmann, under my own observation."

[4] All violence must be avoided, and after each ligation, particularly of the vagi, time enough must be allowed for the animal to become quiet, and the consequent disturbance of the heart's action to cease.

After fright, by a blow on the table . . .	68
Immediately after	64

Experiment on a full-grown Female Rabbit; number of beats in fifteen seconds; five enumerations.

At rest.	After exposure of nerves.	Both sympathici tied.	Both sympathici divided.	Both vagi tied.	Both vagi divided.
58	54	59	68	74	77
57	56	59	65	73	76
57	56	58	64	74	75
57	56	60	64	75	78
57	56	60	65	74	79

I have obtained similar results by experimenting on the cat, in which animal the sympathetic and vagus run nearer together than in the rabbit, but yet are more widely separated than in the dog.

Experiment on a full grown Cat; number of beats in fifteen seconds; five enumerations.

At rest.	Sympathici tied.	Vagi tied.	Ten minutes after.	Twenty minutes after.
40	48	62	68	67
42	45	62	66	66
43	45	58	65	67
43	45	59	67	68
42	47	57	64	67

Many of these trials prove to me conclusively that the sympathici exert an influence on the heart similar to but less powerful than the vagi—retarding its actions, but never increasing them—since the movements of the organ are constantly increased when removed from their influence. That ligation does not exert so much influence as division, I conclude is from the fact that some of the fibres retain their conducting power after application of the ligature.

In using electrical irritation on the sympathetic the objection could be made that, however perfect the isolation, the vagus would also be affected, through the union of the fibres of both nerves in the cardiac plexus. This objection has no weight when ligation and division are resorted to.

In what manner do emotions, such as fright, affect the movements of the heart? This is beautifully shown by the vibrations of the needle. If a violent blow is given on the table, the animal, of whatever kind, draws itself together, the needle remains still for a momentary period (mostly less than a second), and then, in the following seconds, give a series of rapid vibrations, as the following trials show.

A full grown female rabbit had constantly, when at rest, 62 to 63 beats of the heart in 15 seconds; during the same periods of time it had—

	Beats.		Beats.
After a blow on the table	70	Immediately after	63
" " "	69	" "	63
" " "	69	" "	63
" " "	67	" "	63
" " "	67	" "	62
" " "	66	" "	63
" " "	68	" "	63[1]

That this communication between the brain and the heart is exercised by the vagus has appeared probable; yet possibly it may take place through the spinal column, and from thence in the course of the sympathetic, or it might be the result of the general muscular movement attending the shrinking together of the body. The apparatus affords means of proving conclusively that it is the vagus; after the division of the sympathici, the momentary cessation of

[1] Thus in seven observations the most striking similarity.

the vibration of the needle is observed, and the consecutive increase; but after division of the vagi the animal draws itself together as before, but the movements of the needle are not checked, and the number of beats of the heart continues the same. If we observe the effects of emotions on ourselves, we experience the same results; in consequence of a sudden fright, we have the same momentary cessation of the heart's action, and followed by a number of rapid beats. It is a precisely similar occurrence.

Some experiments on the *rami descendens hypoglossi* in the dog, have given no decided results, yet these nerves do not appear to be entirely without influence on the movements of the heart.

The number and intensity of the movements of respiration, and their relation to the beats of the heart have also been taken into consideration in our experiments. They are not without influence; nevertheless, they do not affect the principal results, as given above.

The following is a synopsis of the results obtained:—

1. The needle introduced into the heart affords a more reliable means of enumerating the beats of the heart than auscultation or any other mode; after a little practice, a number of observers in counting from fifty to sixty beats in fifteen seconds vary only one or two.

2. Immediately after the introduction of the needle, the number of beats are increased from two to six, in fifteen seconds; but in a few minutes this subsides; often no increase at all takes place. This may, perhaps, be accounted for by the needle sometimes wounding a branch of a nerve.

3. The number of beats of the heart in fifteen seconds varies in different individuals of the rabbit kind; most are between fifty and sixty; sometimes above sixty; very seldom as low as forty.

4. By irritation of both vagi with a powerful current and consequent cessation of the movements of the heart, the full vibrations of the needle are stopped, but yet slight, rapid, and trembling vibrations occur, which are the indication of partial contraction of single bundles of fibres.

5. Cessation of the heart's action is most surely occasioned, and for the longest time, by irritation of the medulla oblongata, with an interrupted current; the least surely through irritation of one vagus.

6. An uninterrupted current applied to the vagi occasions increased movements, whether passed in a centrifugal or centripetal direction. But this result is not so certain as the opposite effect with an interrupted current.

7. By counting through periods of fifteen seconds, it is found that the movements are somewhat slower than natural immediately after the removal of the poles, but they gradually increase until in the third period the usual number is reached.

8. Irritation of one or both sympathici, with an interrupted current, produces constantly decrease of the heart's action, from one-fourth to one-third, or even one-half the usual number, and, as is generally the case, with an uninterrupted current passing through the vagi, the respiration is also retarded.

9. Ligation and division of the sympathici, and of the vagi, cause (with some exceptions) increase in the number of beats; often in a definite proportion. The increase, on tying both sympathici and vagi together, amounts to one-fourth or one-third the usual number.

10. Fright has a similar effect as galvanic irritation, but weaker, the channel of communication being the vagi, after the division of which the influence of the brain on the heart is lost.

[We are indebted for the translation of the above paper to Dr. JOHN C. REEVE, of Dayton, Ohio.]

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2. *Osmotic Force.*—Prof. GRAHAM in a Bakerian lecture (*Med. Times & Gaz.*) delivered at the Royal Society, presents the following views respecting this power, which are highly interesting in a physiological point of view:—

"The term 'osmotic force' is applied to the power by which liquids are impelled through moist membrane and other porous septa, in experiments of endosmose and exosmose. Diffusion and capillarity are shown to be insufficient to account for it.

"The nature and *modus operandi* of the chemical action producing osmose remains still very obscure. Salts and other substances, capable of determining a large osmose, are all chemically active substances, while the great mass of neutral organic substances, and perfectly neutral monobasic salts of the metals, such as chloride of sodium, possess only a low degree of action, or are wholly inert. The active substances are also, relatively, most efficient in small proportions. The chemical action must be different on the substance of the membrane, at its inner and outer surfaces, to induce osmose; and according to the hypothetic view, which accords best with the phenomenon, the action on the two sides is not unequal in degree only, but also different in kind. It appears as an alkaline action on the albuminous substance of the membrane at the inner surface, and as an acid action on the albumen at the outer surface.

"The most general empirical conclusion that can be drawn is, that the water always accumulates on the alkaline or basic side of the membrane. Hence, with an alkaline salt—such as carbonate, or phosphate of soda—in the osmometer, and water outside, the flow is inwards; but with an acid in the osmometer, on the contrary, the flow is outwards, or there is negative osmose, the liquid then falling in the tube."

A table is given exhibiting the osmose of substances of all classes.

"It may appear to some, that the chemical character which has been assigned to osmose takes away from the physiological interest of the subject, in so far as the decomposition of the membrane may appear to be incompatible with vital conditions, and that osmotic movements must therefore be confined to dead matter. But such apprehensions are, it is believed, groundless, or, at all events, premature. All parts of living structures are allowed to be in a state of incessant change of decomposition and renewal.

"The decomposition occurring in a living membrane, while effecting osmotic propulsion, may possibly, therefore, be of a reparable kind.

"In other respects, chemical osmose appears to be an agency particularly adapted to take part in the animal economy. It is seen that osmose is peculiarly excited by dilute saline solutions, such as the animal juices really are, and that the alkaline or acid property which these juices always possess is another most favourable condition for their action on membrane. The natural excitation of osmose in the substance of the membranes or cell-walls dividing such solutions seems, therefore, almost inevitable. In osmose there is, farther, a remarkably direct substitution of one of the great forces of nature, by its equivalent in another force—the conversion, as it may be said, of chemical affinity into mechanical power. Now, what is more wanted in the theory of animal functions than a mechanism for obtaining motive power from chemical decomposition as it occurs in the tissues? In minute microscopic cells, the osmotic movements, being entirely dependent upon extent of surface, may attain the highest conceivable velocity.

"May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link which certainly intervenes between muscular movement and chemical decomposition?"

—

3. *Ammonia Evolved in Respiration.* By Professors Viale and Latini, of Rome.—After a series of delicate experiments, conducted with the greatest care and minuteness, these eminent chemists have arrived at the following conclusions:—

1st. That ammonia is evolved in each act of expiration. They consider that a healthy man expires, in 24 hours, 0.763 grammes of ammonia; and in a year a quantity equal to 278.504 grammes of the gas. They calculate that in a city containing 160,000 inhabitants, 44.56 kilogrammes of ammonia are evolved by respiration in the course of a year.

2d. That the ammonia is given off in the form of a subcarbonate.

3d. That the lungs cannot expire *pure* carbonic acid gas.

4th. That the *azote*, pronounced by chemists to be a product of respiration, is merely ammonia.

5th. That this ammonia keeps up the supply of the gas existing in the atmosphere, which, dissolved in rain, falls upon and enriches the earth.

6th. That the active principle of contagion is probably merely an ammoniacal salt. Their reasoning on this point is rather curious: The lungs, skin, and excretions all give off a considerable quantity of ammonia, and contagious epidemics are generally developed where a large number of people are crowded together within a small space, or where the emanations of putrefying animal substances are rife. The latter emanations are always ammoniacal. Fumigations of chlorine, chloride of lime, nitric and acetic acids, are the antiseptics which decompose the ammoniacal salts, and probably also the contagious principle. Heavy rains, and tempestuous winds have been known to arrest the course of epidemics of contagious diseases. Viale and Latini believe that the ammoniacal salts which constitute the principle of contagion may be united either to carbonic acid, or to the metalloids (as sulphur, phosphorus, iodine, etc.), or to animal products, generating cyanogen, or to fatty bodies, fixed and volatile oils, or even to albumen, so as to act as a ferment.

7th. That during respiration a certain quantity of albumen is eliminated.

Professor F. Liebig, of Berlin, has published a letter in which he attaches great weight to the foregoing deductions, and considers the demonstration of ammonia in respired air as a fact of great scientific importance.—*Monthly Journal of Medicine*, January, 1855, from *L'Union Médicale.*

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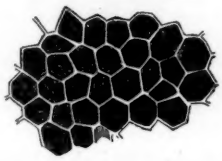
4. *Reticular Mucous Membrane of the Alimentary Canal.*—Erasmus Wilson, Esq., has recently made some investigations relative to the structure of the mucous membrane of the alimentary canal, and has published the following minute account of the reticular mucous membrane, illustrated by two very beautiful figures.

"In the *stomach*," he observes, "the mucous membrane is reticular; the raised portion of the membrane forming a fine network (reticulum) over its whole

Fig. 1.



"A portion of the mucous membrane of the stomach magnified seventy-five times. The alveoli measured 1-200ths of an inch in length, by 1-250ths in breadth; the width of the septa being 1-1000ths of an inch. The smaller alveoli measured 1-250ths of an inch in length, and 1-300ths in breadth. The trifid or quadrifid division of a small artery is seen at the bottom of each alveolus, and in the depressions between the divisions of the artery, the apertures of the gastric follicles; two, three, or four in each depression.

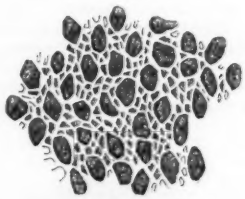
surface, and constituting the septa, the areolæ of the network being the alveoli. The amount of projection of the reticular frame, in other words, the depth of the septa is about $\frac{1}{500}$ of an inch; and their breadth, that is, the thickness of the septa, divested of epithelium, about $\frac{1}{1000}$ of an inch. In structure the reticulum is composed of a fold of the limitary membrane, containing the transparent granular substance of the surface of the corium and a plexus of capillary vessels. The capillary plexus of the reticulum is single, a disposition which probably determines the thickness of the septa; its meshes are large and open, and the vessels flexuous and serpentine in course. The border of the septum is formed by a capillary vessel which sometimes runs in a straight direction, and forms an even rim to the mouths of the alveoli, like that of a honey-comb; but more frequently makes abrupt curves and loops, which project before them the limitary membrane, and give rise to flat papillæ. These flat papillæ are chiefly found at the angles of the reticulum, and their existence in numbers gives a fringed appearance to the rims of the alveoli.

"The areolæ of the reticulum or alveoli are polyhedral in figure, for the most part hexagonal, and slightly oblong; about $\frac{1}{500}$ of an inch in depth at the sides, and somewhat deeper in the middle, and larger at the brim than at the

fundus. Divested of epithelium, they measure in diameter at the brim between $\frac{1}{600}$ and $\frac{1}{250}$ of an inch in length by $\frac{1}{250}$ and $\frac{1}{600}$ in breadth, and at the fundus something less. The floor is uneven, and presents in the centre the axis of a minute artery, from which two, three, or four, commonly three, capillary vessels pass off to make their way to the base of the reticulum, and constitute its plexus. This little capillary axis naturally forms as many ridges in the floor of the alveolus, leaving shallow depressions or foveolæ between them; and at the bottom of these foveolæ, generally three in number, are seen the openings of the gastric follicles. The openings of the gastric follicles are oval in shape, about $\frac{1}{1200}$ of an inch in long diameter, and disposed irregularly in the foveolæ, two or three in each; so that the entire number of gastric follicles opening into each alveolus would amount to from six to ten or twelve.

"*Large Intestine.*—The surface of the mucous membrane of the large intestine presents this most obvious difference from that of the stomach, namely, smoothness—a difference which is as apparent to the naked eye as to the eye armed with the microscope. With the microscope, moreover, we are struck with the symmetry of the reticulum and alveoli, by the greater breadth, lesser degree of prominence, by the flatness and evenness of the septa, and by the elliptical form and shallowness of the alveolar spaces.

Fig. 2.



"A portion of the mucous membrane of the large intestine, magnified seventy-five times. The alveoli measured 1-250ths of an inch in length, by 1-450ths in breadth; the septa between the alveoli measuring 1-600ths of an inch in width. The alveoli are less regular in form and shallower than those of the stomach; and in the bottom of each is a gland with a central excretory aperture. In some of the larger alveoli there are two glands.

"The septa measure about $\frac{1}{600}$ of an inch in breadth; and the alveoli $\frac{1}{250}$ of an inch in length by $\frac{1}{450}$ in greatest breadth. The septa contain a plexus of capillaries, smaller in size than those of the septa of the stomach, and having minute areolar spaces. The brim of the septum exhibits a small, chain-like plexus, composed of two or three of these capillaries lying parallel with each other; the meshes of the plexus are oblong, and about equal in breadth to that of the capillaries. The alveoli are shallow, the floor being slightly convex, and occupied by a mucous gland having a central excretory aperture; in some of the longer alveoli there are two glands and two excretory openings.

"Immediately around the solitary glands, the alveoli and reticulum undergo some modification; the former are larger, measuring $\frac{1}{175}$ of an inch in length by $\frac{1}{250}$ in breadth; and deeper—so deep, in fact, that their floor is no longer visible. The septa are thinner, measuring $\frac{1}{600}$ of an inch in breadth, and furnished with a single capillary, instead of the triple chain of the rest of the membrane."

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5. *Single Kidney.*—M. Labe found the left kidney absent in an old man who died at Bicêbre. There was neither a renal artery nor vein on the left side, and the bladder showed no trace of a left ureter having existed. The right kidney was considerably hypertrophied, and occupied its normal position.—*Comptes Rendus de la Societé de Biologie.*

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6. *Congenital Absence of Pericardium.*—Dr. Bristowe read to the Pathological Society of London, Jan. 16th, 1855, an account of a case of this kind.

The left pleura was continuous with the pericardial sac, the heart lying in the cavity of the former, and in parts adhering to the lung itself. A small rudimentary pericardium was connected with the right pleura, and opened by a small orifice into the left. The patient had died of disease of the heart.

7. *Direction of the Uterus in Little Girls.*—M. A. Porchat, Interne to the Foundling Hospital, in an interesting note read to the "*Societé de Biologie*" in August last states "That the great number of autopsies daily made in the Foundling Hospital has given me the opportunity of investigating the direction of the uterus in very young female children. The greater number of those examined were about two years of age: some were only a few days old. In the majority the uterus was antiflexed, and I am inclined to believe with M. Boulard, that this direction most usually exists. Nevertheless, I have several times seen retroflexion. In some instances the retroflexion was very evident. I have presented to the Society a uterus so retroflected, that the superior portion had fallen back so as to become nearly parallel to the inferior portion. But retroflexion is not always so marked; the specimen which I presented, was pronounced by high authority to be simply retroverted, it being considered that there was no twisting of the neck of the organ. I believe still that the body and the neck formed an obtuse angle, and that the uterus was therefore retroflected. The direction of the uterus depends in some degree upon the situation of the rectum; when the rectum deviates much to the right, the uterus falls directly backwards; but it may, under other circumstances, undergo a lateral deviation. The appendages of the uterus occupy very different positions. The flexion of the uterus is not in my opinion the result of cadaveric alterations, and the degree of flexion has no relation to the distension of the intestine by gas. I have seen the uterus flexed in subjects where the intestine was not distended at all; and it, therefore, is probable that the cause of its direction is to be sought in the particular disposition of the tissue of the organ. The uterus in infants is soft, and falls naturally upon itself; and we have no right to regard this circumstance otherwise than as normal, and we cannot attribute to the organ any one fixed position. In the greater number of instances, however, it was antiflexed.—*Gazette Médicale de Paris*, Nov. 25, No. 47.

ORGANIC CHEMISTRY.

8. *Gastric Juice.*—Grunewald and Schroeder have made some observations on this fluid obtained from a peasant woman affected with fistula of the stomach. The woman was otherwise healthy, 35 years of age, weighed 53 kilogrammes, and was, at the time of observation, nursing a child; the fistula had existed for two or three years, and was caused probably by a perforating ulcer of the stomach. The *quantity* of the gastric juice, after abstraction of the saliva (65 grammes per hour), was computed at 584 grammes (=19 oz. nearly) for every hour, or 14,016 kilogrammes (=22¾ imperial pints nearly) for the day, which is about the fourth part of the weight of the whole body, a proportion much larger than that given by Bidder and Schmidt (6·4 kilogrammes for an individual weighing 64 kilogrammes);[1] 1 kilogramme of weight of a man would secrete, according to this calculation, 264 grammes (=8½ ounces nearly) of gastric juice in twenty-four hours. The smallest proportion was obtained in the morning (state of fasting), being, however, even then not less than between 40 and 400 grammes per hour. The fluid collected at that time was, in general, thin, serous, colourless, and clear; at other periods it was more viscid, containing sometimes bile, without any accompanying signs of functional derangement; not rarely sarcina was detected in it by aid of the microscope. As regards the *chemical constitution*, according to the examination performed

[1] Die Verdauungssaefte u. der Stoffwechsel. Mitau and Leipzig, 1852, p. 36.

by C. Schmidt, the fluid obtained at an early hour of the morning (state of fasting) was either neutral or slightly alkaline; after the ingestion of food it was always acid. *Hydrochloric acid* was not found in any of the analyses of the various portions of the gastric juice secreted at different times of the day, but *butyric* acid and the presence of *lactic* acid became highly probable. The following table shows the numerical proportion of the single constituents; for the sake of comparison we add the figures given by Bidder and Schmidt for the gastric juice of the *dog* and *sheep*.

	MAN. Gastric juice mixed with saliva.	DOG. Gastric juice without saliva.	DOG. Gastric juice mixed with saliva.	SHEEP. Gastric juice mixed with saliva.
Water	956.595	973.062	971.171	986.147
Total solids	43.405	26.938	28.829	13.853
Organic substances . .	36.603	17.127	17.336	4.055
Inorganic substances . .	6.802	9.811	11.493	9.798
Single substances:				
Hydrochloric acid . .	—	3.050	2.337	1.234
Chloride of potassium .	—	1.125	1.073	1.518
Chloride of sodium . .	4.633	2.507	3.147	4.369
Chloride of calcium . .	—	0.624	1.661	0.114
Chloride of ammonia .	—	0.468	0.537	0.473
Phosphate of lime . .	0.961	1.729	2.294	1.182
Phosphate of magnesia .	0.260	0.226	0.323	0.577
Phosphate of iron . .	0.006	0.082	0.121	0.331
Potash, belonging to the organic substances	0.363	—	—	—

The organic substances consisted of coagulable albuminous substance (pepsin), sugar, butyric acid, uncoagulable proteinaceous substances, and lactic acid. The organic acids are not considered as originally present in the secretion of the stomach, but as products from the ingested aliments; they are changeable in quantity according to the quality of food. The hydrochloric acid, on the contrary, was considered as an essential component, although it is not exhibited by the analyses, probably on account of its being so easily bound by the alkalies of the saliva. In an analysis performed at a period, the results of which are communicated in the appendix, Schmidt actually found free hydrochloric acid, but only in the proportion of 0.2 in 1000 parts—a proportion ten times smaller than in the dog. Concerning the question, whether the gastric juice prevents the saliva from transforming starch into sugar (as asserted by Bidder and Schmidt) the authors found that the action of the saliva is not annihilated; they confirm, however, the observation of Bidder and Schmidt, that no sugar is detected in the stomach of the dog even after the ingestion of boiled starch.

The digestion of *proteinaceous food* was examined by introducing weighed pieces of coagulated albumen, meat, &c., in thin linen bags through the fistula into the stomach, and observing both the loss of substance within a certain space of time, and the changes in the microscopic structure of the elements. Thus it was found that, for proteinaceous substances, the dissolving power of the gastric juice of man is far inferior to that of the dog; their solution is completed in the stomach of the dog within two to four hours, in that of man it requires nineteen to twenty hours. Raw meat is better digested by the stomach of man than boiled meat, and veal better than beef.

Concerning the *microscopic alterations*, they found after one hour and a half the primitive fasciculi easily separable from each other, without change in themselves, the sarcolemma destroyed; after two hours and three quarters, the primitive fasciculi commenced to exhibit transverse fissures; after three hours, only transversely striated lamellæ were visible; after three hours and a half, besides the quadratic lamellæ, many primitive fasciculi, fissured in longitudinal and transverse direction, frequently dentated at the extremities; after three hours and three quarters, the primitive fasciculi twice or three times divided in the longitudinal direction; after four hours and a half, scarcely any

solid remains left in the stomach, except some few primitive fasciculi much fissured in the longitudinal and transverse direction, still, however, exhibiting the transverse striæ. After three and a half to four hours the stomach is, in general, empty; the proteinaceous substances become therefore subject to the influence of the enteric secretion, &c. As regards the digestion of *fat*, the membrane of the cells was dissolved in the stomach; but the fat itself did not undergo any alteration. *Milk*, after three quarters of an hour, formed a thick coagulum, inclosing a large quantity of milk globules and free fat; after two and a half hours, the casein was seen partly as amorphous substance, partly in transparent, membrane-like pieces, with some unchanged milk globuli; after three hours and three-quarters scarcely any remains were left in the stomach.

These alterations, which required in the stomach of this woman three hours and three-quarters to four hours and a half, were completed in that of a dog within two hours.

Finally, the authors allude to the great importance of the secretion of the gastric juice for the intermediate circulation of fluids and the intermediate change of matter, the principal object of which is to economise in the animal household, by prolonging the series of changes to which animal matter is submitted within the organism, and thus to render it for a longer time efficient in the discharge of vital processes. We find ourselves more compelled to adopt this view, as the quantity of the secretion amounted, in this instance, to one-fourth of the weight of the whole body.—*B. & F. Med.-Chirurg. Rev.*, Jan. 1855.

9. *Pancreatic Fluid.*—Kroeger gives the results of some experiments performed by Prof. Bidder on this fluid. Through a fistula in the pancreatic duct they collected the pancreatic juice during certain spaces of time at various periods of the day and under various influences. Concerning the quantitative relations, 1 kilogramme of dog secretes, on the average, within twenty-four hours, about 89.3 grammes of pancreatic juice. According to this calculation, an adult man (weighing 64 kilogrammes) would secrete, in twenty-four hours, 5.715 kilogrammes, *i. e.*, nearly the eleventh part of the weight of the whole body. The ingestion of food exercises great influence over the secretion, the latter becoming much increased in quantity almost immediately after meals, reaching its maximum within about half to three-quarters of an hour after the meal, when it is about six or ten times larger than it had been just before the ingestion of food. Water has not the same effect: on the contrary, when taken simultaneously with solid food, it prevents the latter from causing so evident an increase. The concentration of the pancreatic juice appears frequently diminished in the same measure as the quantity is increased, but this phenomenon is not a constant one; and, at all events, the absolute quantity of solid substances is greater after meals than before.

As regards the physical and chemical character of the secretion the author does in general, agree with Bidder and Schmidt;[1] some differences, however, must be noticed. The specific gravity is, according to Kroeger, 1.01065, while Bidder and Schmidt had assumed 1.0306, and Frerichs 1.0082.[2] Further differences become apparent by comparing the following figures relating to 1000 parts of fresh juice.

	Kroeger. (Analysis by C. Schmidt.)	Frerichs.	Bidder & Schmidt.
Water	981.52	986.40	.900.76
Solid matter . . .	18.48	13.60	99.24
Organic substance	12.68	3.50	90.38
Inorganic substance	5.80	10.12	8.86

Concerning the *physiological action* of the juice, its power of transforming starch into sugar is not doubted. According to the author's experiments, 1 gramme of the fresh juice transforms within half an hour, under the influence

[1] l. c. p. 244.
[2] Wagner's Handwoerterbuch der Physiologie, iii. p. 844.

of a temperature of 35° C., 4.672 grammes of dry starch into sugar; as 1 gramme of fresh juice contains 0.014 grammes of pancreatic ferment, one gramme of this ferment would transform 333.7 grammes of dry starch. If we assume, with Frerichs, that an adult man requires daily about 490 grammes (=15 ounces nearly) of starch to compensate the daily loss of carbon, the quantity of pancreatic juice necessary for the transformation of this starch into sugar would be less than 105 grammes, while the quantity actually secreted amounts to more than 5000 grammes. Kroeger is therefore of the same opinion with Bidder and Schmidt—viz: that the pancreatic juice cannot have as its *principal* function the transformation of starch into sugar. As one of the functions, he considers the promotion of the constant interchange of fluids within the body, in the same manner as Bidder and Schmidt have made it so probable concerning the saliva, and with Grunewald and Schroeder concerning the gastric juice. It further appears to him that an intimate connection exists between the secretion of the stomach and that of the pancreas—namely, that the hydrochloric acid secreted by the former is, after having performed its part, neutralized by the soda of the latter, thus again forming the chloride of sodium previously disunited by the process of secretion. In favour of this theory Kroeger observes that the hydrochloric acid secreted by 1 kilogramme of dog through the gastric juice in twenty-four hours amounts to 0.305 grammes, while that of soda contained in the pancreatic juice of twenty-four hours is calculated at 0.237 grammes, *i. e.*, very nearly the equivalent (0.259) corresponding to 0.305 grammes of hydrochloric acid.—*Ibid.*

MATERIA MEDICA AND PHARMACY.

10. *On Digitaline.*—Prof. Christison states (*Monthly Journal of Medicine*, January, 1855) that his experience of the effects of digitaline has been highly satisfactory. I have, he says, used it expressly as a diuretic only; but its sedative virtues have also come incidentally under observation; and it is quite evident that digitaline is a most energetic remedy in both ways.

In the first two trials made with it, which were both of them in cases of extensive, protracted, obstinate œdema in connection with disease of the kidneys, diuresis commenced, in one towards the close of the second day, and in the other a day later; in both the flow of urine was profuse: and in both the œdema entirely disappeared, but with the slowness not uncommonly observed in this form of dropsy.

Here I cannot help observing in passing, that a striking illustration was presented, on both these occasions, of the unsoundness of the objection which has been brought by many practitioners, since it was first propounded by Dr. Osborne, against the use of diuretics in the renal forms of dropsy. I have taken every opportunity, in my clinical lectures, and in occasional publications, to protest equally against the unsoundness of the theory and the looseness of the facts, which have led to the prohibition of diuretics in renal dropsy, and which have deprived many persons of the most immediate and most efficacious means of relief from the principal secondary affection occurring in Bright's disease of the kidneys. And I may here repeat, in the strongest terms, that I have never, except in a single instance, seen the albuminosity of the urine, or any other indications—which either the other conditions of the urine or any other local symptoms can furnish—increased under the employment of such diuretics as I have given in this disease. These are digitalis, squill, and bitartrate of potash, sometimes singly, more commonly combined. What may be the case with other diuretics I shall not pretend to decide. I strongly suspect that they are all in the same category, and that they have been shunned on grounds purely theoretical and baseless. But at any rate I can answer for the theory not holding in the case of the three diuretics I have mentioned. So, too, in the case of digitaline. It is not a little remarkable that in the first two patients to whom

I administered it, the albumen, instead of being increased, was quickly and greatly diminished. In one it disappeared entirely in a few days, and did not return so long as the patient remained under observation in convalescence. In the other it also disappeared: but after some days the albumen reappeared, though in a greatly diminished proportion. Digitaline, therefore, while it stimulates the kidneys to increased secretion, has not, in so doing, any effect, as some have thought of digitalis and diuretics at large, in increasing the peculiar renal irritation which constitutes or occasions Bright's disease.

Digitaline has proved equally, or even more serviceable in dropsy connected with disease of the heart. It has, in my hands, accomplished complete discharge of the dropsical effusions, and thus effected such relief as to enable the patient to return to his occupations, though given in circumstances apparently very desperate. In this respect digitaline has done no more than is often accomplished by foxglove itself. But it has appeared to me to act with more speed, and with greater force after the action did begin. In one instance great depression of the heart's action was brought on instead of a flow of urine.

I have also given it in some local dropsies, more especially ascites, either simple or combined with anasarca of the lower part of the body only. It has not proved more useful, however, as a diuretic in these cases than digitalis and other diuretics; which in general fail to influence the urine or remove the dropsical effusion, when given internally. I have not yet tried it externally, according to the method recommended by me with infusion of foxglove.[1]

There can be no doubt that digitaline possesses the action of foxglove itself upon the heart and circulation. I have not made express trial of this action. But while using it to excite diuresis in a case of dropsy with diseased heart, the pulse began to flag, soon fell to 44, and became very soft and somewhat irregular. At the same time languor, nausea in a slight degree, and faintness ensued. All these symptoms disappeared in a few days after the remedy was discontinued. On this occasion I observed that the two actions of digitaline, its diuretic and sedative actions, do not concur. It is very likely that they are even incompatible. This I have formerly stated to be a probable fact in regard to the actions of foxglove.[2] The disregard of it is, I apprehend, the reason why some underrate the efficacy of foxglove as a diuretic. Attention should therefore be paid to the fact in using digitaline.

The dose to be administered is stated by MM. Homolle and Quevenne at a seventy-fifth part of a grain of digitaline, three times a day. I have never given any other dose. It is a very small one certainly. But a tenth of a grain will kill a little dog; so that the dose is only in proportion to its exceeding energy. The form I have used is that of a pill about a grain in weight. It is to be given with the conditions and precautions observed in using foxglove.

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11. *Note on the Therapeutic action of Cerium.*—By J. Y. Simpson.—The salts of few metals were at present in our pharmacopœia, and Dr. S. saw no reason why many more might not be added to their number. He had recently drawn the attention of the profession to the salts of nickel, which as far as his observation went, presented much similiarity in action to those of iron and quinine, and seemed to be of use in cases of sick headache. He now proposed to read to the Society some very imperfect observations on the therapeutic action of some other metals; and first, as to cerium, which, given in the form of nitrate, and in one grain doses twice or thrice a day, appeared to act as a sedative tonic of considerable value, strongly resembling bismuth, and the salts of silver. He had employed it in the first instance in cases of general chronic intestinal eruption—a peculiar and intractable form of disease, for which arsenic and nitrate of silver were generally prescribed, and where these remedies had failed, cerium had been tried with marked advantage. In irritable dyspepsia, with gastrodynia and pyrosis, and in chronic vomiting, its exhibition was attended with satisfactory results; and in the vomiting which occurs during pregnancy prompt relief was afforded. It was a good tonic, and a useful substitute for the salts

[1] Edin. Monthly Journal of Med. Science, 1850, xi. p. 310.
[2] Dispensatory, 1842, p. 401.

of silver, bismuth, and hydrocyanic acid. Dr. S. had not employed it much in convulsive diseases, as chorea and epilepsy, in which nitrate of silver was used, but the exhibition of the salts of cerium was certainly attended with this advantage, that it could be persevered in without any fear of discoloration of the skin. As far as his experiments with cadmium went, it bore much resemblance to the preparations of antimony, and excited diaphoresis and vomiting. Tellurium, besides its expense, was precluded from being used in practice by its disagreeable effects. Dr. S. mentioned the particulars of a case where a dose had been inadvertently given to a student of divinity, and had been followed by the evolution of such a persistent odour, that for the remainder of the session the patient had to sit apart from his fellow students.—*Monthly Journ. Med. Sci.* Dec. 1854.

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12. *New method of introducing medicines into the system, more especially applicable to painful local nervous affections.*—Dr. Alexander Wood has been led to introduce solutions of morphia and Batley's sedative solution into the cellular tissue, as near as possible to the affected nerve, by means of the small perforating syringe, constructed by Mr. Ferguson of Giltspur Street, for injecting aneurisms with perchloride of iron. Dr. Wood narrated nine cases in which he had employed this method of treatment, in all with perfect safety, in some with complete, in others with partial success. As to the *modus operandi* of this method of treatment, he endeavoured to show, from the experiments of Muller and others, that the effect of the local application of opium to a nerve was to destroy its sensibility at the part, and that from this action of the drug the immediate cessation of the pain arose. He then pointed out the rapidity with which absorption appeared to take place from the cellular tissue, which seemed to account for the rapidity of the narcotic effect which a small dose of opium so introduced was found to produce. He also pointed out, that other medicines might be introduced in the same way.

Dr. W. T. Gairdner mentioned, that a patient in his wards in the hospital, had been injected the other day in the way recommended by Dr. Wood. The result was not decisive, as the complaint for which the man was under treatment, viz: lumbago, had been undergoing rapid amendment, and, indeed, the day after the operation, was nearly gone. The experiment, however, was attended with little suffering, and it was noted that some degree of giddiness was almost immediately produced.—*Monthly Journ. Med. Sci.*, Feb., 1855.

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13. *The So-called Chloric Ether.*—M. Donovan makes some interesting remarks on this article in the *Dublin Med. Press* (Feb. 28, 1855).

"Within a few years," he remarks, "medical practitioners have been in the habit of prescribing what is commonly designated spirit of *chloric ether*. To the compounder of the prescription this name causes some embarrassment: accordingly, we find that sometimes Dutch-oil, sometimes perchloride of carbon, but more generally chloroform, is employed, under the impression that these are the representatives of chloric ether. What is the apothecary to do? He is called on to supply an article which absolutely does not exist—there is no such thing as chloric ether: much confusion, therefore, results; the patient receives no benefit; and the physician administers a medicine which he never intended, and of which he has no medical knowledge.

"It is true that in some treatises on chemistry we find the term chloric ether applied to a liquid which does not deserve that name, it not being an ether at all. Chemists, no doubt, are acquainted with a liquid properly called perchloric ether; but this were rather a hazardous article of materia medica, inasmuch as it detonates violently, either by heating or shaking, and often explodes spontaneously.

"When chlorine and alcohol, in certain proportions, are presented to each other's action, aldehyde is produced. With an increased ratio of chlorine, an ether in very minute quantity is certainly formed, but it is hydrochloric ether, formerly called muriatic ether, not chloric ether."

This ether is difficult to obtain, and is of no use in pharmacy, for at a lower

temperature than that of summer, it assumes the form of vapour unless confined by strong pressure.

Perceiving that spirit of hydrochloric ether is becoming a favourite with medical practitioners, M. Donovan tried several simple methods of producing it, and succeeded by the following:—

"Equal measures of alcohol and liquid muriatic acid, both in as high a state of concentration as possible, were distilled by a cautiously regulated heat through a stoneware worm surrounded by snow, the liquor being received into a balloon refrigerated with a mixture of snow and salt. All the junctures were well secured as soon as the contents of the retort began to boil. Two-thirds of the original measure of the alcohol were drawn off. This liquid was poured into a retort containing a much larger quantity of dried carbonate of potash than was necessary for saturation, and the tubulature instantly closed; for if not instantly, the extricated carbonic acid would have carried off much ether. When one-fourth of the original measure of the alcohol had passed over, the distillation was discontinued. The excess of carbonate of potash retained the chief portion of the water present.

"In this case, when the acid liquor and carbonate of potash were presented to each other, an effervescence took place, because there was water in the former; but for this none would have occurred; and the acid would not have saturated the hydrochloric acid which the ethereal liquor contained.

"Another process, which afforded not only spirit of hydrochloric ether, but hydrochloric ether itself, was the following: A mixture of thirty-two ounces of strong alcohol, and sixteen of sulphuric acid, each by measure, was poured into a retort containing thirty-six ounces of well-dried common salt. The apparatus was arranged as before, and the distillation proceeded with. The first ten ounces of distilled liquor were highly acid. The retort and its contents being removed, a new one, almost filled with well-dried pearlash, was luted to the same apparatus: into this was poured the acid ethereal liquid, and the distillation was resumed. The first two ounces of the distilled liquor consisted of ether strongly impregnated with hydrochloric acid, notwithstanding the presence of so great an excess of alkali, the effect of the latter being merely to detain the water present. Scheele had observed an analogous phenomenon in the case of sulphuric ether (*Essays*, 301).

"This ether was immiscible with water, and burned with a white fuliginous flame—a characteristic of hydrochloric ether. Several modes of detaching the hydrochloric acid were resorted to, care being taken to avoid such means as would produce effervescence. The best mode seemed to be to add a small quantity of water, equal to about one quarter of the ethereal liquor; to agitate the mixture; and when the strata separated, to draw off the upper one with a suction tube. The ether was now entirely free from acidity; but its quantity was much reduced.

"The distillation of what remained in the retort being resumed, the ethereal liquor produced was very acid.

"I found that saturation with magnesia (not carbonate) answered every purpose: but as, on each addition of this, a slight elevation of temperature took place, which caused the expulsion of a little ether in vapour, it was necessary to immerse the bottle which contained the mixture in snow after each successive quantity. This neutralized liquor being redistilled, afforded a spirit of hydrochloric ether of a sweetish penetrating taste. To this was added the ether previously obtained.

"No more magnesia must be used than is necessary for saturation; for if more be present, violent concussions will occur towards the end of the distillation.

"The liquor thus produced contains a small quantity of alcohol; it is therefore *spirit of hydrochloric ether*, and is in a fit state for medical use. It is permanent at all ordinary temperatures. It mixes readily, and in all proportions with water, which pure hydrochloric ether does not. Its taste is pungent and penetrating, but much less disagreeable than sulphuric ether. Of its medicinal properties it is not my province to speak; but from its sensible qualities, I should suppose it to possess energy. I have some reason to suspect that it

would act as an anæsthetic, and perhaps might have its advantages. The season was favourable for these experiments, they having been made during the late fall of snow."

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14. *Chloroform and Ether combined as an anæsthetic.*—M. Robert, in his report on chloroform to the *Societé de Chirurgie*, recommended a mixture of equal parts of chloroform and ether as the best anæsthetic agent. M. Cellaries has subsequently published (*Gaz. Méd. de Montpellier*, 1853), the results of experiments made by him with the compound upon rabbits, etc.; and he reports very favourably as to its efficiency as an anæsthetic. The two fluids combine together, without the occurrence of any precipitate, and the odour of the compound is said to be far from disagreeable.—*Gaz. Méd. de Paris*, No. 96, 1854.

[This combination was first employed, we believe, in this country, and is preferred to either article alone by several of our practitioners.]

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15. *New Hæmostatic Agent Invented by* M. Monsel.—M. Armand, Aide-Major of the 36th Regiment, in an article in the *Gazette Médicale de Paris* (December 30, 1854), states that M. Monsel was induced to undertake the analysis of the eau Pagliari. From his first trials, the reactions made evident the presence of the sulphate of alumina and potash, associated with an organic matter the odour of which resembled that of vanilla. M. Monsel then thought that, on treating the balsam of Peru—which, as is generally known, possesses a strong odour of vanilla—with the subcarbonate of soda, and by throwing the residue of the treatment into a solution of cubic alum, he could obtain an alum charged with a principle identical with that of the analyzed water. This having been verified, he determined to test by the same treatment the resin of the gum benzoin, and the balsam of tolu. The results being the same, he came to the conclusion that the unknown fluid consisted of benzoic acid dissolved in alum. Having formed an hæmostatic agent, in his opinion similar to the eau Pagliari, he proceeded to test its coagulating power experimentally.

The results of these experiments were satisfactory.

M. Monsel has also made another very efficacious hæmostatic, by adding ten grains of tannic acid, and a scruple of alum, deprived of its iron, to an ounce and a half of rose-water.

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16. *New Broth for the Sick.*—By Justus Liebig. To prepare this broth, half a pound of the flesh of a recently killed animal (beef, or the flesh of a fowl) is chopped fine, and well mixed with a pound and an eighth of distilled water, to which four drops of pure muriatic acid, and from half to a drachm of common salt, have been added. After an hour, the whole is thrown on a common hair sieve, and the fluid is allowed to run off without pressure. The first portion, which is turbid, is poured back, until the fluid runs off quite clear. On to the fleshy residue in the sieve half a pound of distilled water is thrown in small portions. In this way a pound of fluid (cold extract of meat) is obtained, of a red colour, and an agreeable taste of broth. The sick are allowed to drink a cupful cold at pleasure. It must not be heated, as it then becomes turbid, and deposits a thick coagulum of animal albumen and hæmatin.

The sickness of a young female servant from typhus, in my house, gave occasion to this preparation. It was called forth by a remark of my medical attendant, that, in certain conditions of this disease, the greatest difficulty which presented itself to the physician lies in an imperfect digestion—a consequence of the condition of the intestines, and the difficulty of obtaining food suitable for digestion and the formation of blood. Generally, broth prepared by boiling is deficient in all those ingredients of meat which are necessary for the formation of the albumen of the blood, and the yolk of egg which is added is very poor in this substance, as it contains in the whole 82½ per cent. of water, and 17½ per cent. of egg-albumen, or a substance analogous to it, and whether this substance, in its nutritive qualities is equal to the albumen of flesh is, according to the investigations of Magendie, at least doubtful. Besides the flesh albumen, the new broth contains a certain quantity of hematin, and therein a large quantity of iron necessary for formation of blood

corpuscles, and, lastly, the muriatic acid for its digestion. A great hinderance to the employment of this broth in summer is its changeableness in hot weather. It undergoes fermentation, as sugar with yeast, without giving a disagreeable odour. What substance causes this change it is very desirable to ascertain. On that account the flesh must be treated with very cold water in a cool place. Icewater and cooling with ice removes this difficulty. But, above all things, care must be observed that the flesh is used fresh, and not several days old. In the hospitals of Munich and in private practice this broth has been employed with great advantage.—*Annalen der Chemie* and *Annals of Pharmacy.*

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

17. *Plastic Bronchitis.*—Dr. T. B. Peacock, Assistant Physician to St. Thomas's Hospital, and Physician to the City of London Hospital for Diseases of the Chest, relates (*Med. Times & Gaz.*, Dec. 30, 1854) a case of plastic bronchitis, in a boy 11 years of age, an out-patient of St. Thomas's Hospital, and presents the following interesting remarks on this disease, which, though rare, is of sufficiently frequent occurrence to render a knowledge of its chief characteristics a matter of practical importance.

"During the last session of the Pathological Society, I prepared a report, since published in the *Transactions*, in which I collected the particulars of thirty-four cases of fibrinous expectoration, and deduced from them certain general inferences as to the circumstances under which it occurs. Since that report was drawn up, a memoir on the subject has appeared, by Dr. Thierfelder, of Leipsic, which contains the account of a case which had occurred to the author, and notices of five others, three of which are new to me. I have also seen the reports of cases by Gendrin and Caseaux; and my attention has been drawn, by Dr. O'Connor, to two interesting communications, containing several well-observed cases, by Dr. Banks and Dr. Gordon, published in the *Dublin Journal*, which, from the unavoidably hasty way in which the report for the Pathological Society was drawn up, had escaped my notice.

"The cases to which I have now alluded, together with those referred to in the report, amount to forty-eight in number; and it would probably not be difficult still further to increase the series. I find, however, that the inferences previously drawn so closely correspond with those which an analysis of the more numerous data establish, that there would be little advantage in endeavouring to obtain a wider basis for generalization.

"1. The *plastic bronchitis*, *bronchite pseudo-membraneuse*, or *bronchitis croupposa*, as the disease has been termed by different modern writers, is of more frequent occurrence in males than in females.

"2. While the affection is not limited to any period of life, but occurs both in the young, in persons of middle age, and in the old, it is most frequent between the ages of 20 and 50.

"3. It may attack persons who have enjoyed robust health, but generally occurs in those who have been previously suffering from some chronic pulmonary disease, or who have been exhausted by other debilitating causes.

"4. The affection is characterized by the ordinary phenomena of bronchitis, aggravated, however, by the difficulty in expectorating the solid material; and it may either be acute, rapidly terminating in recovery or in death; or chronic, the peculiar sputa continuing to be expectorated, at longer or shorter intervals, for weeks, months, or even years; or it may repeatedly recur on any exacerbation of the bronchitic symptoms.

"5. Hæmoptysis, at least to any considerable extent, is by no means a frequent or even a common symptom; but, on the contrary, the membranous material is most generally expectorated, either almost alone, or mixed with the

ordinary bronchitic sputa, usually with the white, glairy, adhesive mucus which first occurs during a bronchitic attack.

"6. The membranous material is most generally of a dull white colour; more rarely of a brownish hue, or slightly discoloured by blood. Chemically and microscopically, it presents the usual characters of fibrin. In form, it may either be moulded into the shape of a bronchial tube and its ramifications, or it may constitute a thin shred of membrane. When assuming the branched or dentritic form, it may be hollow or solid, and is found to be comprised of a number of delicate laminæ, arranged concentrically. The trunk of the branched cast generally has a diameter of from one line to two or three lines, or varies from the size of a small crow-quill to that of a large goose-quill. The branches subdivide till they become very minute. It would thus appear that the casts are generally moulded in bronchial tubes of the third or fourth size; but cases are recorded in which the masses expectorated had a much larger diameter, and in which, after death, the main bronchi have been found obstructed; and others in which the false membranes extended from the smallest tubes to the trachea.

"Michælis supposed that the *bronchial polypi*, as the casts used formerly to be termed, were of two kinds: one consisting of fibrinous exudations from an inflamed mucous membrane; the other being decolorized clots, from blood retained in the tubes after hæmoptysis; and this view was supported by Dr. Cheyne. While, however, the expectoration of solid masses, assuming the form of the bronchial ramifications, occasionally follows profuse hæmoptysis, and so affords support to the idea of Michælis, I believe Dr. Watson to be correct in supposing that the fibrinous material is always the product of inflammation; and the hemorrhage, when it occurs, an accidental complication. In the Report, I have suggested that cases of plastic expectoration may, in some cases, when accompanied by hemorrhage, be analogous to the hemorrhagic pleuritis or pericarditis, the effusion not being limited to the escape of the liquor sanguinis only, but being combined with a discharge of blood-globules; while, in the ordinary form of fibrinous expectoration, the plasma only escapes. In some cases, also, the false membrane may adhere closely to the mucous membrane, and be with difficulty separated from it; in others it may have little or no connection with the exuding surface, and hence another cause of the presence or absence of hemorrhage.

"7. The ultimate result of cases of fibrinous expectoration depends upon the circumstances under which the formation of the solid matter takes place. When, as is not unfrequently the case, the membranous effusion occurs as a complication of phthisis, or of some other fatal disease, it necessarily follows the course, and may but little affect the result, of that disease. When, on the contrary, it is an idiopathic and acute affection, though the symptoms by which it is attended are ordinarily urgent and alarming, it usually soon subsides, under appropriate treatment, and the patient quickly regains his usual health. In other cases, however, the acute symptoms may subside, but the false membrane may continue to be formed, at intervals, for a long period.

"The opinion which I have just expressed of the generally remediable character of the acute plastic bronchitis, is, I am aware, opposed to the conclusion of Dr. Thierfelder, who states, that, in the acute form, the disease is generally rapidly fatal; and refers, in support of his assertion, to the fact, that of thirteen cases which he has collected, and which he very erroneously supposes to be all that have been placed on record within the last 120 years, eleven proved fatal. It is true, that the affection is occasionally attended by fever of a low typhoid type, and proves very rapidly fatal; but most generally the inflammation appears to be by no means intense; the accompanying constitutional disturbance has a sthenic character, and the urgent symptoms subside when the fibrinous material is expectorated. The unfavourable opinion as to the general result which is entertained by Dr. Thierfelder, is owing to the small number of the cases which he has analyzed, and to his having confounded with them cases of acute capillary bronchitis, in which false membranes were found in the tubes after death.

"The case which I have related will be observed to belong to the chronic

form of the disease, the patient having had several attacks before that in which he fell under my notice. Like many of the similar cases, he was a very delicate person, and inherited a predisposition to pulmonary disease. The inflammatory symptoms had probably never been very severe, and had already nearly subsided. The casts appear to have been at first moulded in the smaller bronchial tubes of the upper lobe of the left lung, but, as the disease advanced, the fibrin was either effused from the original seat of disease, in larger quantity, or the disease itself extended, so that the masses expectorated had the form of the main bronchus of that lobe. When first expelled, the solid matter formed oblong or rounded bodies, about the size of a filbert, and these, when macerated for a short time in water, gradually unfolded themselves, till their peculiar branched form became apparent; but for the complete separation and exposure of the smaller divisions much careful manipulation was required. The largest pieces expectorated had an extreme length of about three inches, and at the trunk were three to four lines in diameter. In some I was able to trace ten distinct subdivisions, and the terminal portions were most minute. Two or three masses were generally expectorated on the same day, or within two or three days, and he then usually did not bring up any more for a period of a week or ten days; but, occasionally, they were expelled more frequently, and in a day he once expectorated not less than seventeen pieces. When first observed, the membranous material was only expelled after a severe fit of coughing, but latterly they were brought up with little effort. The physical signs which were observed in the case were those of slight capillary bronchitis, with obstruction in the tubes of the upper part of the left lung, and consolidation of the corresponding pulmonary substance. There was slight sibilant rhonchus, with more or less crepitation, heard in different parts of the chest; some dulness on percussion at the upper part of the left side, especially posteriorly; and slight bronchial respiration, with mucous sounds, occasionally having the clicking or valvular character, more frequently the usual subcrepitation, in the same regions.

"These signs correspond with those generally heard in cases of a similar description. In the early stages of the disease there are the usual signs of bronchitis; and, in the later, these pass into those of obstruction of the smaller tubes and consolidation, probably collapse of the lung.

"M. Valleix states that if, when the voice is unaffected, there exist the signs of capillary bronchitis, but, instead of their being a general and abundant subcrepitant rhonchus, the *petite bruit de soupape* is heard, with sonorous rhonchus, we may suspect the presence of false membrane in the tubes; and he refers, in support of this opinion, to cases observed by Caseaux and Barth; the latter I have not been able to find; but M. Caseaux, in the report of his case, describes a sound, *de soulèvement et d'abaissement;* and it is most probable that, under the term of *bruit de soupape*, M. Valleix means the clicking or valvular sound, which was once or oftener heard in the case here related. Dr. Cane mentions having detected a similar sign in a case which he has reported; and Dr. Gordon speaks of a 'peculiar flapping valvular sound,' and 'double flapping sound,' as heard in some of his cases, and evidently regards the sign as that referred to by the French writers. This subcrepitant or valvular sound cannot, however, be regarded as very characteristic of the presence of false membrane in the tubes. Such a sound may be caused by any viscid material obstructing a large bronchial tube; but the precise nature of the effusion can only be ascertained by inspecting the sputum.

While, however, the diagnosis of the plastic bronchitis can thus not be effected by physical signs alone, there are certain general characteristics which sufficiently distinguish the disease from other forms of affection to which it bears a very close resemblance. It has been confounded with the acute capillary bronchitis; but, though in the latter affection, as in cases reported by Nonat and Fauvel, false membranes do sometimes form in the tubes, and the secretion is always very viscid, this cannot be regarded as an essential feature of the disease. Its most striking character is the tendency of the inflammation quickly to extend itself over the bronchial membrane of both lungs, and so to lead to rapid asphyxia; whereas the plastic bronchitis is generally limited

to a portion only of one lung, and the physical signs of the two diseases are very dissimilar. The false membrane, in cases of acute capillary bronchitis is also of comparatively rare occurrence; I did not once meet with it in any of the cases which I examined during the last epidemic of influenza, and when it does occur, it is rarely more than in small shreds, which gradually pass into the more usual viscid secretion.

"The acute plastic bronchitis may also be mistaken for acute pneumonia, or for a circumscribed empyema; and when, as in cases mentioned by Dr. Gordon, in addition to dulness on percussion, and absence of respiration, the heart also is dislocated, it would be difficult, if not impossible, to distinguish it from the latter disease.

"The plastic bronchitis is analogous to the form of inflammation of the mucous lining of the air-passages, which is known under the names of diphtherite, angina membranacea, and croup, in the adult, and which, commencing in the fauces, is occasionally continued through the larynx, to the terminal portions of the bronchial tubes.

"It may naturally be expected that, in this communication, some allusion should be made to the cause of the chief peculiarity of this form of disease, and that some reason should be assigned why the mucous membrane, when inflamed, instead of pouring out the usual secretion, exudes a fibrinous material. I regret, however, I have no new views to advance on this point. It is easy to state objections to the various explanations which have at different times been proposed, to show that it cannot depend on the age of the individuals attacked, nor on the intensity of the inflammatory action; that there is no proof that it is owing to the excess of fibrin in the blood, as supposed by some pathologists, or to the particular tissue affected, as imagined by others; and that the views of Reynard and Stokes, that the lining membrane of the bronchial tubes loses the mucous, and partakes more of the serous character as it approaches the periphery of the lungs, is not borne out by recent investigations. I am, therefore, compelled to resort to the unsatisfactory mode of solving the difficulty, by supposing that the exudation of fibrin depends on the specific character of the inflammation, resulting from some peculiarity in the constitution of the individuals affected, though I cannot hazard a conjecture as to the nature of this peculiarity.

"The only other remarks which I have to offer upon the case which I have read, refer to the treatment which was adopted. Before the patient fell under my notice, the period for active interference had passed, and the indications to be fulfilled, consisted in the relief of the slight bronchitic symptoms still existing, and the prevention of the further formation of the fibrinous material. For the first purpose, expectorants, anodynes, and counter-irritants were employed; and for the second, tonics, and especially the cod-liver oil; and I have, within the last few days, seen the patient, and have found him tolerably well, and free from cough or expectoration. How far, however, his recovery will be permanent it is impossible to say.

"In the cases of acute plastic bronchitis which come under the care of the medical practitioner at the commencement of the disease, the aim should be—1st, to relieve the inflammatory symptoms; 2d, to assist the expulsion of the membranes when formed; and 3d, to prevent their further formation. These indications should be accomplished by a judicious employment of antiphlogistic measures, regulated by the urgency of the symptoms and the power of the patient. It would be necessary to cup or apply leeches over the affected part of the chest, to exhibit antimony, and to affect the system mildly with mercury. The expulsion of the membrane might be assisted by the use of emetics, and by inhalations of the vapour of water, or of ethereal or other fluids; and the further formation of the membrane would probably be best controlled by mercury. In the later stage, the use of anodynes and tonics would be required.

"The use of mercury in conjunction with other antiphlogistic means is strongly recommended by Drs. Gordon and Banks; and Dr. Thierfelder, while he says little benefit results in the acute disease from a purely antiphlogistic course, approves of the employment of depletion in combination with mercurials. He also regards mercurials, the hydrochlorate of ammonia, and the

iodide of potassium, as beneficial in the chronic form of the disease. The latter remedy he employed in the treatment of the case which he has related, in very large doses. Dr. Watson, in alluding to a case of plastic sputum, which was attended with hemorrhage to an alarming extent, remarks that, though the case at first seemed to call for active interference, and was in consequence energetically treated, on looking back upon it, he regarded the treatment as having been unnecessarily active; and the remark seems applicable to a large proportion of the cases of this description. Though alarming in their first appearance, they often do not seem to require very active measures for their relief. In those cases in which the disease becomes chronic, or recurs at intervals, on any exacerbation of bronchitic symptoms, protection against the weather, and, if possible, residence in a warm climate, should be enjoined."

18. *Case of arrested Pulmonary Tuberculosis.*—By Dr. STOKES. Some years ago I saw a gentleman, who came to town labouring under all the symptoms of well-marked phthisis. The disease had been of some months' standing, and the patient *was a perfect* picture of consumption. He had a rapid pulse, hectic, sweating, purulent expectoration, and all the usual *physical signs* of tubercular deposit, and *of a cavity* under the right clavicle. I may also state, that the history of the disease was in accordance, in all particulars, with this opinion. I saw this patient in consultation with a gentleman of the highest station in the profession, and we both agreed that there was nothing to be done. This opinion was communicated to the patient's friends, and he was advised to return to the country. In about eighteen months afterwards a tall and healthy-looking man, weighing at least twelve stone, entered my study, with a very comical expression of countenance: "You don't know me, doctor," he said; I apologized, pleading an inaptitude that belongs to me for recollecting faces. "I am," he said, "the person whom you and Dr. ——— sent home to die last year. I am quite well, and I thought I would come and show myself to you." I examined him with great interest, and found every sign of disease had disappeared, except that there was a slight flattening under the clavicle. "Tell me," said I, "what you have been doing?" "Oh!" he replied, "I found out from the mistress what your opinion was, and I thought as I was to die I might as well enjoy myself while I lasted, and so I just went back to my old ways." "What was your system of living?" said I. "Nothing particular," he said; "I just took what was going." "Did you take wine?" "Not a drop," he replied; "but I had my glass of punch, as usual." "Did you ever take more than one tumbler?" "Indeed, I often did." "How many? three or four?" "Aye, and more than that, I seldom went to bed under seven!" "What was your exercise?" "Shooting," he said, "every day that I could go out." "And what kind of shooting?" "Oh, I would not give a farthing for any shooting but the one!" "What is that?" "Duck shooting." "But you must have often wetted your feet?" "I was not very particular about the feet," says he, "for I had to stand up to my hips in the Shannon for four or five hours of a winter's day, following the birds."

So, gentlemen, this patient spent his day standing in the river, and went to bed after drinking seven tumblers of punch every night; and if ever a man recovered from phthisis he had done so when I saw him on that occasion. Suppose, now, that he had been confined to an equable temperature, and a regulated diet, and had been treated in all respects *secundum artem*, what would have been the result? Any of you can answer the question. In point of fact, this very treatment had been adopted during the first three months of his illness, and his recovery may be fairly attributed not so much to the duck shooting and whiskey-punch, but to the tonic and undepressing treatment which he adopted for himself, and which his system so much required to enable him to throw off the disease.—*Med. Times and Gaz.*

19. *Tuberculosis limited to the Base of the Right Lung.*—The following case related by Dr. J. T. BANKS, one of the physicians to the Whitworth Hospital, is interesting, as presenting a very unusual exception to the general rule as to the primary seat of tubercle:—

"A man, aged 40, was admitted into the Whitworth Hospital; he had been ill for some time previously, but had not been incapacitated from following the arduous occupation of a labourer. The symptoms under which he suffered before his admission were cough, hurried respiration, and pain in the right side.

"On being questioned, it appeared that he had never had hæmoptysis, nor, in short, any ailment until his present, and that there did not exist any hereditary taint. A few days before entering the hospital, he perceived that his expectoration had acquired a most offensive odour.

"His appearance was that of a man who had long laboured under disease. His skin was sallow and greasy; his pulse accelerated, never under a hundred; respiration rapid; he complained of debility, loss of appetite and of night-sweats. On examination of the chest the upper regions were found, both on auscultation and percussion, to be perfectly free from any morbid sound. The base of the right lung was dull, and a coarse muco-crepitating rale was audible; there was also an augmentation of vocal resonance. He complained of cough, followed occasionally by the expectoration of matter which was distinctly purulent and of an abominable fetor. After a violent paroxysm of cough he often got up a very large quantity of the purulent fluid, which was always followed by relief.

"He gradually lost flesh and strength; he suffered at times from diarrhœa. The auscultatory signs of cavity at the base of the right lung became manifest, cavernous breathing was audible, with gurgling and pectoriloquy, which was so marked as to be almost painful to the ear of the auscultator. The purulent fetid fluid continued to be expectorated in great quantity, and after a residence of seven weeks in the hospital, he sank under the influence of his wasting malady. The apices of the lungs anteriorly and posteriorly, up to the close of life, were free from any sign of diseased action.

"The autopsy revealed the existence of tubercular infiltration of the base of the right lung, with numerous cavities. In no other part of the lung was there a trace of tubercular deposition.—*Dublin Hospital Gazette*, 15th Feb., 1855.

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20. *The cracked pot sound (bruit de pot fêlé) is not always indicative of a cavern.*—Prof. Bennett states that, "On examining a man called John McKay, on the 2d of November, 1854, it was determined on percussion of the apex of the left lung anteriorly, that it was unusually resonant, and accompanied by distinct cracked pot sound, of a clanging character. On auscultation over the part, the inspiration was harsh and exaggerated, the expiration prolonged, and the vocal resonance very loud, amounting to pectoriloquy. This unusual combination of physical signs attracted my attention. The rest of the lung, as well as the condition of the opposite one, together with the symptoms, could leave little doubt that this man was in the last stage of phthisis pulmonalis. The disease followed its usual progress, both lungs being ultimately affected, and the apex of the left one always presenting on percussion the same physical characters as have just been described, without moist rattle of any kind. He died January 6th, 1855, and on the following day his body was examined. Both lungs were scattered throughout with miliary and infiltrated tubercle, and the intervening pulmonary tissue was loaded here and there with simple exudation. At the apex of the left lung was a mass of bluish-gray miliary tubercles, about the size of a hen's egg, closely condensed together, very hard to the feel, and cutting under the knife with considerable resistance. This mass was solid throughout, tolerably limited, the pulmonary tissue around being light and spongy, containing a few very small isolated tubercles. There was no cavity or softened tubercle in this left lung, although in the opposite one, a few small anfractuous excavations existed here and there in the upper lobe.

According to Skoda,[1] "the cracked pot sound is heard in the thorax, over tolerably large and superficially situated cavities which contain air, and communicate with the bronchial tubes. When the percussion is forcible, or the thoracic walls flexible, the cavity is compressed at each stroke, and a portion

[1] Markham's translation, p. 21.

of air suddenly driven out of it into the bronchial tubes; this hissing murmur, caused by the escaping air, is mixed up with the ordinary percussion sound of cavities, and this compound represents the cracked pot sound." But the observation made in the case of McKay, has satisfied me that occasionally distinct cracked pot sound may be elicited over condensed lung, without any cavity whatever. In referring to an excellent paper on this subject by Dr. Markham,[1] I find that on one occasion, both he and Dr. Sibson have noticed this phenomenon, over the upper portion of a lung, which was afterwards shown to be gorged with blood and serum, though still retaining some portion of air. It would seem, from what has been said by Skoda, Stokes, Walshe, as well as by Dr. Markham, that a peculiar tympanitic sound may be heard over collapsed or condensed lungs, when covered or mingled with a certain amount of air. For instance, when in cases of pleuritic exudation, air is effused into the pleura, a few hours before death—when in certain cases of pneumonia there is also emphysema, etc. In the case I have alluded to, these conditions were so far fulfilled, that the tubercular mass described was surrounded by spongy lung full of air. At all events, it must be evident, that the physical conditions on which this peculiar sound depends, require more careful study, and that our ideas as to its necessary connection with a cavity, must undergo modification. —*Monthly Journ. Med. Sci.*, Feb., 1855.

21. *Absence of Chlorides from the Urine, Diagnostic of the Onward Progress of Pneumonia.*—In the April number of the *Monthly Journal of Medical Sciences*, Prof. Bennett gave an account of certain facts which confirmed the statements of Simon, Redtenbacher, Beale, and others, as to the absence of chlorides in the urine during the onward progress of pneumonia. The professor resumed these inquiries on returning to the clinical wards of the Edinburgh Hospital last summer, and in the number of the above mentioned Journal for December last, he relates three cases of the disease there treated which confirm the diagnostic value of this change in the urine. In all these cases the absence of chlorides marked precisely the onward march of the pneumonia, whilst their presence indicated its cessation and was generally accompanied by the returning crepitation and commencing absorption of the exudation.

22. *Pathology of Saccharine Assimilation.*—Dr. Gibb read a paper on this subject before the Medical Society of London (Jan. 27, 1855), of which the following is an abstract:—

Before entering upon the pathology, he gave a brief summary of the physiology of the subject to the present time, and referred to the discoveries of Bernard, Pavy, and himself. He considered that the relation subsisting between fat and sugar, more especially in the liver, and as likely to help in the working out of the pathology of fatty degeneration, was a discovery, the importance of which could not be over-estimated. He gave a table of the pathological conditions of saccharine assimilation, and considered some of the more important. In diabetes, he supported Prout's view of the formation of quantities of sugar in the stomach, which, owing to paralysis of the converting function was absorbed into the circulation. This condition he looked upon as the result of the paralysis of the saccharine functions of the liver, which he believes to be arrested in this disease, and not over-exerted, as Bernard believes. The principal evidence he brought forward in support of this view was the invariable absence of sugar in the liver after death, and the "starved" state of the liver, from its poorness in fat. In fatty liver only did he consider the saccharine function abnormally active. The gorged condition of the portal venous system, found after death, he looked upon as owing to hepatic obstruction, due to the paralysis of the special function of the liver, from taking too much gastric sugar to that organ which it could not assimilate. If thoracic disease was a frequent termination of diabetes, it was due, he believed, to the want of the naturally formed hepatic sugar to undergo combustion, and not from excessive combustion of the sugar, as Bernard maintains. He admitted, however, that

[1] Monthly Journal for June 1853, p. 495.

diabetes and allied affections were under the governing power of the medulla oblongata and its branches, and sympathetic communications. One of the uses of healthily-formed sugar in the liver, independent of combustion in the lungs, he said, was to form fat; and he believed it to be one of the great sources of the fat throughout the system; and that, if sugar was convertible into fat, fat could not be reconverted into sugar; and he supported this view by reference to experiments. He next considered oxaluria, in connection with sugar, and believed there is a mutual relationship existing between the two, but not constant. In dwelling upon diseases of the liver, he mentioned that fatty liver was the only abnormal condition which did not interfere with its healthy function; but here it was increased, and, as the lungs did not consume it proportionate to its secretion, it was converted into fat in the liver. He drew an analogy between this condition in the marine mammalia, marine birds, etc., and the same condition in man; in the former it was a normal process, while in the latter it constitutes disease. He related some cases of disease in the body, accompanied with fatty liver, to prove that the severity of a disease elsewhere does not necessarily destroy the saccharine function of the liver. In one recent instance he found sugar in fatty liver, coincident with cancer of the same organ. In other diseases of the liver sugar is absent; in cholera, sugar is more easily detected in this organ than in health. In relation to nervous affections and saccharine assimilation, he believed there was a very great sympathy between the two, perhaps more so than in other classes of disease. He confirmed the observations of Dr. Goolden, having found the urine saccharine in many nervous affections; he had found this fluid saccharine in epileptics, even when convulsions were threatened, but which passed off; also in pertussis, both in its simple and complicated forms. He briefly went over the special conditions of the base of the brain, as likely to influence saccharine assimilation. From the length of his paper, the author was only able merely to allude to the influence of dyspepsia, gout, boils, and carbuncles, Bright's disease, tuberculosis, abscesses, diseases of the respiratory system, cholera, milk, and the effects of certain medicines, upon saccharine assimilation. The peculiar effects of each were shown in a large diagram.

Dr. Goolden expressed his concurrence in the views entertained by the author, and mentioned that in an analysis of the urine of a large number of patients suffering from nervous disease, sugar was discovered in many of the cases; but very recently, the analysis having been continued by the same person (Dr. Dundas Thompson), sugar was not found to anything like the same extent. The cholera in 1832 was preceded by a greater amount of saccharine matter in the urine of patients. He (Dr. Goolden) had traced many cases of diabetes to injury of the brain. One resulted from the kick of a horse, and another from the falling of a piece of timber on the patient. In both these cases the quantity of urine was diminished from twelve or fourteen to about five pints, below which it was difficult to reduce it when the quantity was at first considerable, even after the saccharine principle had disappeared. The kidneys, once accustomed to secrete large quantities, continued to do so. His treatment had not been so successful as he at first had anticipated, but in many cases he had materially relieved the patients. In his experience animal diet did not diminish the quantity of urine, and he always allowed his patients milk and farinaceous food *ad libitum*. He saw no benefit to be derived from prohibiting fluids. Opium influenced the amount of sugar in some cases, but not in all.—*Med. Times and Gaz.*, Feb. 3, 1855.

23. *Nature and Treatment of Diabetes Mellitus.*—Dr. F. Headland read, February 10, last, before the Medical Society of London, a paper on this subject. After a slight general sketch of the symptoms, he proceeded to the inquiry, What is the physiological cause of this abnormal and excessive secretion of glucose, or grape sugar? Three chief theories had been brought forward to account for this.

1. *Theory of Renal Disorder.*—By Dr. M. Good and others of his time it was supposed that the glucose was formed by the kidney in the act of secretion. The author discussed the various alleged morbid conditions of the kidney in

diabetes, none of which are known to be constant. The discovery of sugar in the blood and other secretions of diabetics is sufficient to overthrow this theory.

2. *Theory of Saccharine Assimilation;* held by Bouchardat in France, and by the majority of physicians in England. It supposes that the formation of glucose is due to a deranged digestion or assimilation. Most consider that it is formed in the stomach; others blame the liver. This notion, also, the author disclaimed—arguing that, after a meal on starchy matters, grape sugar may be found in the blood of a healthy man; that it is part of the function of the liver to form sugar and fat out of albuminous compounds; and that this explanation does not account for the excretion of sugar, for grape sugar given to a healthy man does not pass out in the urine.

3. *Theory of Saccharine non-Assimilation.*—Supported by Mialhe, Liebig, B. Jones, and others. To this the author gave his own adhesion. It derives confirmations from the experiments of Lehmann, Dumas, and D. Thomson. The starch of the food is the chief supporter of the respiratory process. Starch cannot be absorbed without being first changed into dextrine. This is a sort of transition to grape sugar, into which it is all formed in the blood. This grape sugar is not yet in a condition to be oxidized; it is therefore again changed into two atoms of lactic acid (or some very similar material). This then combines with oxygen in the blood, supporting the animal heat by its combustion, and forming carbonic acid. These changes require certain agents, probably ferments, to effect them. Supposing they have proceeded as far as glucose, and the agency be wanting which should change this into lactic acid—then the glucose, not being available to the system, is excreted in the urine. The liver attempts to supply the want by forming glucose and fat out of albuminous food. This glucose passes also into the urine. In addition to all this waste, the very tissues are preyed upon to supply fuel for the respiration. The author then discussed at length the subject of treatment, under the following heads:—

A. *Erroneous Plans of Treatment.*

1. Attempts to prevent the formation of glucose.
2. Attempts to hinder the excretive function of the kidney.

The first is a natural process; the second is a healthy provision.

B. *Doubtful Plans of Treatment.*

1. The use of diuretics.
2. Stimulation of the nervous centres, as by strychnia.
3. Treatment directed to the liver.
4. The use of oxidizing agents.

C. *Correct Plans of Treatment.*

Dietetic Rule.—To supply, if possible, such articles of food as shall be able, at the same time, to nourish the patient, and to maintain the respiratory combustion without passing through the stage of glucose. (Among other things, fat and oils, dry wines, and milk, were recommended.)

Therapeutic Indications—

1. To give some remedy that shall seem to be capable of causing the glucose to undergo its normal transformations. Yeast, rennet, pepsin, etc., were discussed. The author particularly recommended *milk just turned sour*, as containing a decomposing casein, which transmutes milk sugar into lactic acid. He had advised the use of this remedy in his *Essay on the Action of Medicines*. It should be used as an article of diet; or it may also be given in enemata, and in warm footbaths.

2. To replace the urinary secretions by means of diaphoretics and purges. (The author recommended copious sweating, for a physiological reason.)

3. To attend to the general health, and to treat complications.—*Med. Times and Gaz.*, Feb. 17, 1855.

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24. *The Juvantia and Lædentia in Diabetes.*—John M. Camplin, Esq., in a letter to Dr. Bright, read before the Royal Medical and Chirurgical Society January 9, 1855, gives some particulars of his own case, believing that the means which proved beneficial to himself may afford hints useful in the treat-

ment of others. It was nearly ten years since the author was attacked with diabetes. The symptoms appeared to have been of unusual severity, and many of his professional friends viewed his case as all but hopeless. The principal interest of the paper consists in some modifications of diet, which, the author thinks, contributed most essentially to his recovery. In addition to the usual nitrogenous diet recommended to diabetic patients, and, as a substitute for bread, he first tried a cake or biscuit made of washed flour and lard; but this article soon disagreed. He then tried gluten bread; but, after a year or two, it became so unpleasant as to be insupportable, and he abandoned it, and never resumed it. Dr. Prout recommended him a bran cake, but the coarse particles of the half-ground bran acted on the bowels, and could not be continued in that form. The author devised a hand-mill, by which he succeeded in grinding the bran into a very fine powder, and in making a kind of cake, the use of which he was enabled to continue for some years. The author gave a formula by which this cake may be made, and is the same in substance as the directions contained in a foot-note in Dr. Pereira's last edition of the *Materia Medica*. The author believes that the bran cake greatly ministered to the prolongation of his life, and is desirous of stating his opinion, that some modification of this bran cake might be made on a large scale, and be used with equal success in our hospitals. The use of fat meat and eggs the author found productive of biliary derangement, which, although they might have exercised some unfavourable effect on the constitution, did not appear to augment the diabetic symptoms. The author spoke of the propriety of using curds instead of butter, and the advantage he derived from this preparation from fresh cow's milk. With regard to vegetables, he limited himself at one period exclusively to the cruciferæ. In respect of tea and coffee, the author gave the preference to the former; and, as a beverage at dinner, very dilute brandy and water; or, if wine was employed, it should be claret, as he found other wines injurious. He alluded to the remedial influence of cold and tepid sponging of the surface of the body, and the effects of warm bathing, not omitting the benefit to be derived from efficient clothing. The author's experience of other cases, as well as from his own, led him to the conclusion that there was no specific for this disease; for the cases he had witnessed had improved under the use of various remedies, keeping in view the great principle of building up the system, promoting the action of the skin, regulating that of the liver, and, as much as possible, removing the causes which aggravate the disease. Among remedies, the citrate of ammonia in the effervescing form, combined with the citrate of iron, afforded him at one period of the attack very sensible relief. The author was inclined to class the disease among the neuroses, believing that exhaustion of nervous power existed in all cases. In his own person, two causes had been long at work; the wear and tear of a laborious profession, and an ill-regulated diet; but he was not desirous of raising any discussion on this point, as his aim in the present communication had been simply to relate the facts of his own case, and the results of actual experience.

25. *Diagnosis of Typhous and Typhoid Fevers.*—By Dr. Parkes. You are brought to see this young woman, we will say, for the first time; the specific rose-spots are gone; she is labouring to all intents and purposes under severe bronchitic and chest symptoms (a chemist, or practitioner with a druggist's shop, has prescribed, and given cough mixtures, perhaps, without seeing her); you find her respiration 30 in a minute, cough incessant, with some expectoration, nervous symptoms also well-marked; vertigo complained of, torpor, the eyes closed; she is delirious at night; she has also diarrhœa, pain over the abdomen, pulse quick, tongue furrowed and somewhat coated. Suppose, I say, you were called to such a patient, and moreover she is unable to give any account of the previous illness, how are you to make the diagnosis? There are only two ways—one the positive method, the other the method as it is called by "exclusion." The first is obvious enough, and will of course be more valuable to the practised eye of the experienced physician, who seizes the nature of the case at the first glance by a sort of intuitive knowledge of what typhoid really is. Now the method of diagnosis by exclusion—the plan of logic-writers, *per viam exclusionis*, in this and other

diseases, is one, though not without disadvantage, one yet of no mean importance. The first question you resolve in your mind will be—is she or he, as the case may be, labouring under any of the idiopathic fevers? any of the exanthemata? No. Is it typhous? You make the same answer, as the eruption in ty-*phus* is as different from ty-*phoid* as scarlatina from measles. The eruption is absent in patients under 22 or 21 (this patient's age is about this). Is it relapsing fever, so common some years, as 1828–29? No. You ask yourself, then, is it typhoid? Yes. Nervous symptoms are marked, chest symptoms and diarrhœa also; the latter loose, granular, yellow, so peculiar to typhoid. You have soreness of the right iliac fossa; but then you say we have no rose-spots, and then you remember in at least 20 per cent. these rose-spots are not found. You must weigh and balance all these circumstances in your mind.—*Med. Times and Gaz.*

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26. *Non-identity of typhous and typhoid fever.*—Although typhous and typhoid fevers resemble each other very much in their external appearances, Professor Forget of Strasburg believes that they are clinically and essentially different in their nature. They are anatomically different; they are not equally frequent in their occurrence; and typhoid fever may arise from many causes, quite irrespective of infection. In follicular enteritis the typhoid condition is not constantly present, and it is usually secondary; while in typhous, the prostration and stupor, attending its invasion, indicate a powerful intoxicating principle, acting primarily as a poison, which is not usual at the commencement of the typhoid state. Gastro-intestinal symptoms are more constant and primitive in typhoid than in typhous fever; for in it they are occasionally found existing alone; and we very rarely see cases of follicular enteritis which do not present the rough (*saburral*), punctulated, and rosy tongue; also *gargouillement* and pain in the right iliac region, and either diarrhœa or constipation. Though other symptoms may coexist with them, they are never wholly obscured. In typhous again, especially at its outset, the tongue is frequently moist, smooth, and white; meteorism and abdominal pain are absent, and there is no derangement of the process of defecation. If we except the rosy, lenticular spots, peculiar to typhoid fever, and which constitute, according to M. Forget, its most constant differential sign, there are no symptoms in general connected with either malady which are not purely accessary and inconstant. Typhoid fever is usually slow, and gradual in its course, and in simple uncomplicated cases its evolution is in exact ratio to the development of the intestinal lesion. In typhous, on the other hand, the disease is very violent at first, and afterwards becomes suddenly or alternately better or worse, and often in those cases which appear the most unfavourable, it advances rapidly and spontaneously to a favourable termination. Ataxia characterizes it more than it does typhoid fever.

As regards the treatment of the two diseases, Professor Forget says that while, in typhoid fever, this must always be adapted to the peculiarities of each particular case, special attention must be paid to the intestinal ulcerations which are present during its course, and that all those medicines should be avoided which are calculated to aggravate these lesions of the alimentary canal. There is no need for this caution in cases of pure typhous.—*Monthly Journ. Med. Sci.*, Feb., 1855, from *Gaz. Med. de Paris*, Nos. 42–48. 1854.

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27. *Some Points in the Treatment of Gout, Rheumatism and Rheumatic Gout.*—Dr. Fuller read a paper on this subject before the Western Med. and Surg. Soc. of London. He commenced by stating, that in all diseases, but especially in those which, like gout, rheumatism, and rheumatic gout, are attributable to faulty assimilation and the consequent formation of a morbid matter in the blood, the success of treatment must necessarily depend upon the care and judgment bestowed on four several points—first, the history of the case; secondly, the appearance and general condition of the patient; thirdly, the character of the excretions; and, fourthly, the selection and adaptation of proper remedies to meet the various exigencies of the case. He then proceeded to point out the bearing of each of these considerations upon the course of treatment to be pursued, and after drawing attention to the close relationship

subsisting between the amount and character of these various excretions and the condition of the system generally, he insisted on the necessity of providing for the full and perfect action of all the excretory organs. He instanced cases which had fallen under his care at St. George's Hospital, in which the long continuance of the disease was evidently referable to the sluggishness of some one particular organ; and others in which the obstinacy of the symptoms was attributable to the imperfect action of all the excretory organs, rather than to extreme sluggishness in any one of them. These latter cases, when not attended by fever, and when characterized by affection of the muscular structures, are often benefited by bark and hydrochlorate of ammonia, which, under such circumstances, has the effect of promoting an increased flow of all the excretions, and so of eliminating the *materies morbi* and checking the disease. Dr. Fuller, however, dwelt especially on the fact, that under different conditions of the general health, very different remedies are needed to regulate the various excretions. He showed, that when the system is low and deficient in vital energy, tonics, such as bark, quina, sarsaparilla, steel, and cod-liver oil, together with change of air, change of scene, freedom from anxiety, and the use of proper baths, and a due amount of exercise, form the most effectual means of checking the mal-assimilation on which the formation of the poison depends, and of increasing the action of the excretory organs, whereby its elimination is brought about; and that, unless such remedies are exhibited, and the tone of the system is improved before colchicum, guaiacum, and other so-called "specifics" are administered, no good results can be produced. He attributed the failure of treatment, in many instances, to want of care in tracing out the history of the case, whereby it happens that purely neuralgic pains, as well as the pains which follow the incautious use of mercury, and the pains and swellings resulting from a gonorrhœal taint, are confounded with gout and rheumatism, and are wrongly treated accordingly. He regards the too frequent habit of treating disease in accordance with its name, rather than according to the condition of the patient and the character of the secretions, as another cause of failure, inasmuch as it leads to the practice of prescribing each of the so-called specifics separately, and apart from other remedies. He pointed out, that, in most cases, the action of all the excretory organs is disordered; and that, whereas no single remedy will excite them all equally to action, the judicious practitioner should employ such a combination of remedies as will prove efficacious for the purpose. Dr. Fuller combated the opinion advanced by Dr. Garrod as to the non-existence of such a disorder as rheumatic gout; and avowed his belief that the three forms of disease which form the subject of his paper are referable to different forms of mal-assimilation and the consequent formation of different poisons in the blood; and he urged the remarkable differences observable in the age, sex, and physical condition of the sufferers from rheumatic gout, in the ascertained causes of the disease, the mode in which it makes its attacks, its symptoms, its behaviour under treatment, and the pathological changes to which it gives rise, as evidence in favour of its being a disease essentially distinct both from gout and rheumatism. He admitted the value of Dr. Garrod's uric acid test as a means of distinguishing, in most instances, between gout and rheumatism, as well as between true gout and rheumatic gout; but he did not admit the absence of uric acid from the blood as proof of the disease being pure rheumatism. Moreover, admitting the value of the test theoretically, he denied its practical utility. Its only indicational value is in regard to the administration of colchicum, and even in this respect it cannot be trusted. In no cases is lithic acid more abundant in the blood than in those old cachectic cases of gout, accompanied by the formation of chalk-stones in the joints and by granular degeneration of the kidneys, yet in none is colchicum of less avail or more ill borne by the system. On the other hand, when the blood is free from lithic acid, as it usually is in true rheumatism, colchicum is often of great service, by acting as an evacuant and influencing the secretions of the kidneys, liver, and bowels. Colchicum, therefore, should not be relied upon in all cases of gout, neither should it be discarded in all cases of rheumatism; and in the one as in the other, the history of the case, the condition of the patient, the stage of the disease, and the cha-

racter of the excretions, must be our guides to the administration of that or of other remedies.—*Med. Times and Gaz.*, Feb. 3, 1855.

28. *New remedy for Pruritus Vulvæ.*—Dr. SHOLZ recommends an Indian plant, the *Caladium seguinum*, which is used by the natives of India as an anaphrodisiac, for the treatment of those cases of this most distressing malady, which are due rather to a hyperæsthetic condition of the parts, than to any lesions of the mucous membrane of the vulva. The plant belonges to the *Aroideæ*. Scholz has used it with great success in two cases which had previously resisted all remedies; and he administered it in the form of an alcoholic tincture in doses of six drops.—*Arch. Gén. de Méd.*, Sep., 1854.

29. *Anaphrodisiac properties of Bromide of Potassium.*—THIELMANN recommends this remedy as an excellent anaphrodisiac in satyriasis, in the frequent and painful erections during gonorrhœa, in spermatorrhœa, and in nymphomania. He administers it in doses of from 2 to 3 grs. every two or three hours; and, at the same time, enjoins a mixed vegetable and milk diet, and forbids all acid substances —*Med. Zeit. Russl.*, 1, 1854.

30. *Sulphate of Quinine in Phthisis.*—D. MUNTENDAM, a Dutch physician, has just published a paper to show that, from his experience in twenty-two cases, sulphate of quinine, given along with acetate of morphia, or even alone, is capable of prolonging the life of the patient in many cases of phthisis, and that it may even effect a cure in those cases in which a tubercular deposition has just commenced, especially in married women and children. He alleges that, when given continuously in small doses, it does not cause dyspnœa, diarrhœa, headache, or any disagreeable effects; and he believes that in very many, but not in all cases of phthisis, it should be ranked as one of the best remedies for the disease.—*Monthly Journ. Med. Sci.*, Feb., 1855, from *Nederlandsch Lancet.*

31. *Borax injections in Infantile Diarrhœa.*—M. BOUCHUT considers infantile diarrhœa to be of two kinds; the one, symptomatic of lesions of the intestinal mucous membrane; the other, idiopathic, a nervous or catarrhal flux from the great intestine, which may occasion death, without leaving any material morbid appearances. In the latter variety, M. Bouchut recommends clysters containing the bi-borate of soda, as peculiarly efficacious. This remedy proves as beneficial as it does in aphthæ of the mouth, it acts as a weak astringent on the intestinal mucous membrane, and as an alkali in neutralizing the acid secretions poured out by it, which lead secondarily to ulceration of the bowels, and especially of the anus.

He uses clysters containing from 10 to 20 grammes of the salt, in 125 grammes of water.

In cases of infantile diarrhœa symptomatic of intestinal ulcerations, M. Bouchut has found benefit from the use of borax, administered internally in the dose of 2 grammes, in 80 grammes of a mucilaginous emulsion.—*Gaz. des Hôpitaux*, 16th Sept., 1854.

32. *Trial of Phosphorus in Paraplegia.*—Prof. BENNETT has given phosphorus in seven cases of paraplegia from disease of the spinal cord, in the form of phosphuretted oil (4 gr. of phosphorus dissolved in ℥j of olive oil.) In none of these cases has he been able to satisfy himself that any improvement was occasioned. He commenced with three drops a day, which were afterwards cautiously increased to ten, and in one case to fifteen drops. But these large doses soon induced violent nausea and vomiting, and, after a short suspension of the remedy, he continued it in doses of three drops.

In the case of chronic myelitis, which took ten and fifteen drops, the phosphorus was excreted by the lungs, as the breath smelt strongly of the drug, but was not phosphorescent at night—a phenomenon which has been seen by some physicians who have employed it. In another case, that of a delicate woman, with probably congestion of the cord, a large amount of phosphate was passed in the urine presenting, under the microscope, beautiful feathery crystals.

The dose of the oil, therapeutically, ought never to exceed five drops, and even this amount cannot be administered for any length of time without deranging the stomach.—*Monthly Journ. Med. Sci.*, Feb., 1855.

33. *Therapeutic Action of Lupulin.*—Debout's observation on the favourable influence of lupulin in quieting painful erections induced Dr. Zambuco to institute some experiments with the medicine, which, to prevent mistakes, he administered to the patients with his own hands. The form was threefold; pure lupulin, the tincture, or mixed with an equal part of sugar. The doses were different: of pure lupulin, 1—16 scruples (these high doses caused no disturbance of the nervous system); the tincture, 2—16; the mixture with sugar, 2—3. From eight well-marked cases of painful erection, consequent on gonorrhœa, out of a much larger number, the author was enabled to draw the following conclusions: Lupulin possesses a remarkable effect upon the genital organs; the erethisms were quieted in four-fifths of the cases. It is of therapeutic utility when there exists the necessity for keeping the penis in a state of rest. Camphor given in large doses, with a similar object, both irritates the alimentary canal, and often fails in its end. The sedative and anti-blennorrhagic influence of lupulin depends upon a resinous, æthero-oily principle; while the bitter principle yields a real tonic. The author has seen lupulin given with good effect in scrofula.—*Med. Times and Gaz.*, Feb. 3, 1855, from *Schmidt's Jahrbuch.*, 1854, No. XII.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

34. *Curability of Cancer, and its Diagnosis by means of the Microscope.*—[In our preceding No., pp. 224–231, we gave the first portion of the discussion at the Academy of Medicine on this subject, and we shall now present the continuance of it.]

M. Jules Cloquet thought, with M. Velpeau, that the use of the microscope in the diagnosis of cancer, threatened to divert professional attention from clinical observation. The essential nature of cancer—not only of the encephaloid, but of all other varieties—was to reproduce itself with all its primitive characters. He had operated on a great many epithelial cancers of the lips, almost all of which had recurred; and he had removed many fibro-plastic tumours, several of which had returned. Cancers varied from one another only in their degrees of malignancy and fatality. He regarded the melanotic as the most formidable species, and could remember no instance in which he had removed a cancer of this kind without its having recurred. Next in order to it in malignancy he ranked the encephaloid, fibro-cartilaginous, fibrous, colloid, cartilaginous, epithelial, and fibro-plastic varieties. The danger of reproduction varied according as the cancer was encysted or ramified, ulcerated or not so, complicated or not with glandular engorgements, &c. We were constantly finding our prognosis to be fallacious in cases of cancer. In illustration of this he cited several cases in which malignant cancers of the breast of unfavourable prognosis had been removed, without any recurrence of the disease.

He thought the following conclusions were warranted: 1. That microscopical examinations should never be allowed to supersede clinical observation; but that both should, in every case, be resorted to. 2. That tumours, having all the characters of cancer, could occasionally be removed without recurring, although their prognosis was most unfavourable. 3. That other tumours might return, the circumstances of which were apparently the most favourable. 4. That although there were cases in which an operation seemed clearly indicated, and others in which it was as clearly counter-indicated, there were many others in which the most experienced practitioner could not decide the question with any degree of certainty or satisfaction.

October 24.—M. Robert.—My remarks on this occasion will still continue to be strictly practical in their nature, and I shall endeavour to demonstrate clinically the utility of the distinctions pointed out by the microscope—in the use of which I confess myself to be by no means perfect, but the employment of which I have several times found to be highly useful for the elucidation of doubtful cases.

First of all, let me allude to M. Velpeau's address. In it he stated that there are statistics on record which are directly opposed to those of Munro, Mayer Boyer, Macfarlane, &c., which I quoted. He alludes to those of Hill and Flajani; but these, in my opinion, are not worthy of being placed in opposition to those which I cited. But even granting that they do counterbalance the statistics I quoted, this does not in the least affect the evidence borne by the experience of Lebert and Broca. The conviction which I have expressed, therefore, seems to be supported by the opinions both of ancient and of modern surgeons.

The cases brought forward by M. Velpeau from his own private practice are divisible into two classes: 1. Those of a remote date (of the nature of which I have expressed some doubts), and 2d. Those of recent times; while the cures were not of more than six, five, three, and two years, at the publication of his *Maladies du Sein*. Those of the *second* class I frankly accept as *true* cases of cancer, having not only been pronounced such by an authority in whom I have every confidence, but having also been demonstrated to be of this nature by means of the microscope. The cases included in the former and more numerous class have not been defended with any great warmth by M. Velpeau. I am still of opinion that M. Velpeau has not yet waited long enough to be able to announce the *radical cure* of those rare cases of *true* cancer in which he has operated. We know that recurrence may take place after a long space of time, and M. Velpeau has himself given two instances of distant recurrence in his work, in one of which it happened at the end of fourteen, and in the other at the end of twelve years. But further, M. Velpeau has detailed four cases, one of which occured in 1847, the others in 1848 and 1850, in which tumours containing cancer cells were removed, and the patients recorded as cured. Chance has thrown in my way intelligence respecting one of these cases; a lady, who was seen by one of the first surgeons in Paris, and who died last year of a relapse of her malady.

As to the clinical infallibility of M. Velpeau, he grants that there are difficult cases, and, when he says that he is never deceived, he means in the most marked cases of cancerous or other tumours. In this respect, however, he is no farther advanced than the rest of us. But in the two extreme phases of the evolution of mammary tumours, for example, our diagnosis is surrounded by almost insuperable difficulties. How can we always distinguish, with certainty, a small encysted mass of an encephaloid nature, which is mobile, unadherent, and which rolls below the fingers, from an incipient hypertrophic tumour? Who has not met with such cases? But let us, on the other hand, suppose the case of an enormous ulcerated tumour, with everted and fungous edges, with profuse fetid suppuration, the hemorrhages from which have undermined the general health—and all this occurring simultaneously with general cachexia. M. Velpeau has said that he would declare such a tumour to be cancerous, suppose all the microscopes in the world were to testify against him. I deprecate this singular mode of demonstration. Less confident or more inquisitive than M. Velpeau, I appealed to the microscope in a case similar to this which I met in 1849, and it showed me that this horrible tumour was composed only of the hypertrophied mammary tissues. I operated, and the patient has ever since enjoyed perfect health. How precious to us, then, is a method by which we may rectify our first impressions! No one has a higher estimate of the sagacity of our eminent colleague than myself, but after clinical examination of a morbid tumour with the naked eye, he is often, like ourselves, left in doubt as to its true nature. This I can prove. At the last meeting of the Academy, the members were deeply interested by M. Velpeau exhibiting several tumours which he had removed, and the nature of which he had diagnosed before operating. In these cases examinations with the naked eye and by means of the microscope, testi-

fied once more to the ability of our colleague. But one of the specimens puzzled M. Velpeau; and the diagnosis of it then, as well as before the operation, was very obscure. The tumour resembled neither encephaloma, nor schirrus, nor colloid cancer—in spite of the gelatinous aspect of its tissues—nor had it much likeness to an adenoid tumour. M. Velpeau regarded this doubtful tumour as of benign nature, but added that he would never be surprised to hear of the ultimate appearance of a similar tumour elsewhere. Thus, out of six specimens, taken at random, he is unable to determine the nature of one, by ordinary means. In his embarrassment he consults two microscopists, one of whom finds cancer cells, and the other discovers none; and M. Velpeau, as much in doubt as ever, doubts the efficacy of the microscope. Had I been present on that occasion, I confess that my convictions would have been strongly shaken. But I have since then been fortunate enough to obtain accurate and precise information respecting this tumour, from the most competent authorities.

(Here M. Robert read notes, which he had received from MM. Robin, Verneuil, and Houel, which showed that these three able microscopists, after having severally demonstrated identically the same structures in this tumour, had unanimously concluded that it contained no cancerous elements, and that it was merely a particular variety of mammary hypertrophy.)

Since receiving this information, said M. Robert, I confess that I feel much more convinced of the value of the microscope.

The question before the Academy as to the *curability of cancer*, lacks a proper basis. For, to know whether or not a disease is curable, we must know exactly what this malady is; we must carefully distinguish it from those affections which are liable to be confounded with it; in a word, we *must define it.* Hence, on beginning this discussion, we ought to have set out with a clear definition of what cancer really is—which task all the orators have evaded. Owing to the difficulty which has hitherto existed in assigning to this malady precise and pathognomonic characters, we have merely recorded its most striking and constant phenomena; that is to say, as a substitute for a dangerous definition, we have given a summary description of the evils which attend the evolution of cancer. This hiatus is unfortunate, inasmuch as it has led, and must always lead, to the greatest confusion. And hence it happens that the word *cancer*, instead of designating a pathological condition, and its simple varieties, is the qualifying attribute of an immense class of very different maladies. This family is, by common consent, composed of elements so diverse, that in looking to their clinical characters, and keeping their structure altogether in the background, we are obliged to admit (as has been done by MM. Leblanc, Velpeau, and Cloquet) the existence of a great many genera, which are themselves subdivisible into species—thus rendering the classification of limits quite indeterminate and arbitrary.

But, gentlemen, our venerable instructor, Bichat, long ago laid down the following law, which has remained immutable amid all the revolutions of science: When two diseases differ in their symptoms, their course, and their termination, they also differ in their seat, and in the nature of their accompanying lesions; and reciprocally, when the anatomical lesions vary, and when the structure of the pathological products is dissimilar, then the functional disorders which cause their evolution, are distinct in their nature.

Taking this immutable principle, then, for our groundwork in the study of cancerous disorders, if we find that clinical observation does not afford us the elements of satisfactory distinctions, and that their physiological characters are not sufficiently general—admitting of too many exceptions, and containing too many varieties—then it behooves us to study their structure. But are the means afforded us by pathological anatomy, in the present day, sufficient for the prosecution of this study? I unhesitatingly say, No. Persevering and laborious investigations of tumours have doubtless produced very useful documents, but they yielded very insignificant results. To these researches we owe that useless, but copious, phraseology, which at present encumbers pathology; such words as colloid, tuberous, ligneous, napiform, chondroid, pustulous, encephaloid, schirrous, lardaceous, medullary, fungus hæmatodes, cancerous and

phagedenic ulcers, noli me tangere, &c. What signifies to us the hardness or softness of a tumour, or the sound it gives when cut, if not decisive?

There can be no doubt that we still need to be possessed of more delicate characteristics of cancer. But whence are we to get them? Chemical analysis has been unable to give them. It was once hoped that we might gain our end by scraping the surface off a section of a tumour, so as to demonstrate the presence or absence of a particular creamy or lactescent juice. M. Barth attached great importance to this juice, and considered it almost pathognomonic of cancer. But an analogous fluid is found in some tumours which are not at all cancerous in their nature, and these exceptions prove that this test is not by any means conclusive.

Such was the state of science when the microscope came upon the field. It proposed a precise and brief definition for cancer, and declared it to be a malady constituted by the presence of a special anatomical element, which had no analogue in the economy, and the characters of which were easily recognizable by the practised eye. The microscope informs us that *that* only is a cancer which contains the cancerous element; that this element combines with the various simple and composite tissues of the economy, and that it is from the occurrence of this mixture in different proportions that the innumerable varieties of cancer result. It further declares that there are other morbid tissues which, at first sight, are apt to be confounded with cancer, and that their development in the economy often indicates great danger, and may even occasion death. But instead of being constituted, like cancer, by the presence of an anatomical element, without any analogue in the economy, *i. e.*, heteromorphous in its nature—these tissues are merely the result of an exaggerated yet circumscribed growth of one or more of the elementary tissues which enter into the normal constitution of the organism.

Finally, I would have you remember that the laborious microscopical investigators have made their clinical observations keep pace with their histological researches. Their practised eyes having discovered, in these morbid tissues, structural peculiarities which had escaped the earlier observers, they proposed an entire revision of this part of our nosology, inasmuch as they found that those diseases structurally resembled others totally distinct from them in their seat, origin, prognosis, &c.

What ought the partisans of the clinical school to do in the case of so notable a deviation from the great precept of Bichat which we have enunciated? We ought either to expose firmly the errors of the new doctrine, if such exist, or to confess frankly and honestly the progress which the microscope has achieved.

I shall now proceed to state to you what I have learned concerning this question, both from my personal observations, and from the attentive perusal of modern works; and if I succeed in demonstrating to you that cancerous, epithelial, and fibro-plastic tumours are not identical affections, I think I will have sufficiently proved the utility of the microscope, which alone can reveal to us precise and minute distinctions.

You are aware that, according to the microscopists, the cancer-cell is the specific characteristic of cancer. I will not pause to discuss, as M. Velpeau has done, the relative value of the cells, the nuclei, and the nucleoli; for, although I have certain convictions of my own on the subject, I think it is better to leave questions like this to those men who have studied them *specially*, and not to meddle with topics about which we know little practically. You will perhaps say that I am adopting the definition of cancer given by the microscopists, totally forgetting that my opponent refuses to accept their opinions. M. Velpeau has said that the cancer-cell cannot be the specific anatomical character of cancer, because it has been found in non-cancerous tumours. I would ask my learned colleague by what authority he declares the tumours which contained the cells in question, not to have been cancerous? Because, he will reply, they did not return after extirpation. From this it would follow that every tumour which does not recur after removal, although it contains myriads of so-called cancer-cells, is not cancer; and from this I would also be correct in concluding that the so-called cases recorded in M. Velpeau's work, at p. 585, were not cancers at all. And, by carrying this mode of reasoning still further,

I could prove that my colleague has not, any more than Munro and Macfarlan, cured a single case of cancer.

The physiological are not less marked than the anatomical characters of cancer. The disease has a constant tendency to become generalized, and manifested elsewhere, either in the neighbouring lymphatics, in some other organ, or in the viscera themselves, especially in the lungs and the liver. Before cancerous tumours ulcerate, and injure the constitution by profuse suppuration and hemorrhages, we often see the signs of general cachexia become manifested. The malady constantly returns after extirpation, and the progress of cancer becomes much more rapidly fatal in exact proportion to the frequency of its recurrences, and the operations for its removal. I have often noticed this fact, but was never more struck by it than in the case of a woman, aged 38, who, after four successive ablations of a small tumour of the mamma, had three recurrences of the malady in less than two years, and who returned to the hospital three months after the fourth operation, where she died from the recurrence of a multitude of similar tumours within the chest.

Fibro-plastic tumours are always circumscribed, and have not the irregular ramified (*rameuse*) form which characterizes certain cancers, especially schirrus. On their first appearance they possess a firm, slightly elastic consistence; they are homogeneous in structure, and contain no juice. Their tissue is termed by Lebert *fibro-plastic*, and it is composed of cells, and of fibres which exactly resemble the elements of cellular tissue during their development. These tumours advance but slowly, and they are, for a long time, quite compatible with all the appearances of good health. Nevertheless they may become softened, ulcerated, and even generalized, and they may occasion death by being developed in the viscera. Of these facts M. Velpeau has given us several examples. But even among those rare cases in which the malignity of the fibro-plastic tissue rivals that of true cancer, we find consoling facts, which M. Velpeau ought not to have passed over in silence. Of these facts, which are now very numerous, I shall mention a few.

Paget (in his *Lectures on Tumours*, 1853) mentions a case quoted from Maclagan, of a girl, who, in 1832, was operated on for a firm, elastic, fibrous-looking tumour, in the left lumbar region. It returned after a year, and was again removed in 1834. A second recurrence happened after a period of eighteen months, and the third operation was completely successful; for in 1850—fifteen years after it—she was, and had continued until then, in perfect health.

In 1839 I removed, from the right axilla of a young man, a large tumour, the size of both fists, which was circumscribed, lobulated, but firm and elastic, and appeared to be composed of fibrous tissue intermixed with fat. In five years thereafter it returned, and I operated again in October, 1844, removing a tumour, the size of my fist, possessing the very same appearance as the first. Again it returned after the lapse of other four years, and for the third time it was extirpated in January, 1849. M. Lebert examined the tumour, and pronounced it to be fibro-plastic tissue mixed with fat. Since this time the patient has enjoyed perfect health.

Again, Mr. Syme, in 1839, removed a tumour, situated over the first rib, from a man aged 48. A second operation was necessary in 1843, a third tumour was removed in 1847, and a fourth in 1849, and a fifth in 1851. Professor Bennett examined these tumours microscopically, and found them to be composed of fibro-plastic tissue. Shortly after this fifth operation another tumour, of a considerable size, was formed under the clavicle. During all this time the patient's general health was never impaired. Ultimately this tumour ulcerated; hemorrhage took place; and, worn out by it, the patient died in July, 1852. *There was not a trace of the malady to be found in any of the internal organs.*

In September, 1849, M. Chassaignac removed a tumour from beneath the left clavicle. In a few months it reappeared, and a second operation was performed in December, 1850, a third in July, 1851, a fourth in January, 1852, a fifth in July, the same year, and, finally, a sixth in October, 1853. Since that date, M. Chassaignac has not seen the patient. During the course of these operations the general health continued excellent, and there was no implication of any of the lymphatics. The tumours were examined each time by M. Lebert, who

pronounced their structure to be purely fibro-plastic, mixed with a little gelatinous matter.

When we reflect on the course of all these fibro-plastic tumours, on the obstinacy of their recurrences, and on the fatal termination of many of them, we certainly cannot regard them as of a benign nature. But, on the other hand, are we correct if we class them with cancerous tumours? Has an encephaloid or a schirrous tumour ever been extirpated for four, five, or six recurrences, without the disease becoming generalized, and the constitution cachectic? And can the instances of fibro-plastic recurrence in the viscera be compared, for frequency of manifestation, to the more constant return of cancer in the glands, and within the cavities of the chest and abdomen?

I would next speak of *epithelial affections*, which are composed anatomically of epithelium cells or of the epidermis which normally covers the skin and mucous membranes. I cannot conceive how M. Velpeau can say that the structure of such tumours (if we are to believe the microscope) renders them akin to benign growths, such as warts, corns, &c. What does it signify, that two tumours have in common a normal anatomical element, if this element be not disposed and combined in the same manner in each? In warts and corns, the epidermis, though more abundant, stands in exactly the same relations to the other parts of the skin as it does in the normal condition of the parts. We will find, on examination, that this is not the case in an epithelial tumour. M. Velpeau's assimilation, then, is more specious than real.

Epithelial tumours appear principally on tegumentary surfaces, and ought to be studied separately according as they are situated on the skin or the mucous membranes. On the skin they begin by small crusts (*boutons croûteux*), warts, or fissures, and they form ulcers called *cancroid*, on account of their differing in appearance from cancer, progressing more slowly, and having less tendency to infect the system. The utmost evil that they do is to affect the neighbouring glands. When treated by energetic appliances, calculated rapidly to destroy the indurated parts, they often become healed definitely and radically. The epithelial tumours of the mucous surfaces (as types of which I shall specify those of the tongue and cervix uteri) are different in their progress; their course is more rapid; they reappear almost invariably after extirpation, and they terminate by causing death. Their tendency to generalization, however, is very far inferior to that of true cancer. If we examine the bodies of those women who have died of these deep ulcers of the cervix, we may often find the uterus partly destroyed; and the bladder, peritoneum, and great blood-vessels may be perforated by the disease. But, in such cases, we never find epithelial masses deposited in the viscera; the utmost traces of the disease which we can discover are the slightly enlarged glands in the neighbourhood of the uterus. It is the same with epithelial ulcers of the tongue; the glands of the neck alone become affected; and, when the patients die, it is rather from being exhausted by hemorrhages, and from the long-continued deglutition of putrid matters, than from any generalization of the disease having occurred.

At the orifices of the body, where the skin and mucous membrane become blended together, epithelial tumours possess, what I call, *mixed* characters, which they communicate to the complexed structure of the tissues affected. But there, as on the skin, the tumours continue for a long while in the condition of local maladies; and although it may be proved that their extirpation is less frequently curative than of those on the skin, it can also be demonstrated that the radical cures effected by the operation are very highly encouraging. I am quite aware that epithelial tumours *may* become propagated or reproduced in the lymphatic glands, and even in the viscera; but such instances of generalization are very rare.

These, then, are the differences, both of the minute structure and the mode of progress, of the three lesions which are generally confounded together under the term—cancer. We cannot consider epithelial and fibro-plastic tumours as benign in their nature, and we certainly cannot, in point of malignity, rank them with cancerous maladies properly so-called; M. Velpeau does not admit these distinctions, for to him all these lesions are cancers, and he does not recognize the speciality of any of their elements. If the patient *must* sink, "what

does it matter," he asks, "in *what manner* he dies—whether by the disorganization of the diseased parts or from the results of general infection?" I would answer, in the first place, that all these maladies are not alike in their progress and severity; but even supposing that they *were* all equally fatal, is it not important to study them, in order to establish the differences between them, and to endeavour, sooner or later, to apply the knowledge thus gained to their scientific treatment? I feel convinced that, at present, the only question concerning cancer that is at all settled is that of prognosis. Is it nothing for us to know that although the removal of a cancer leaves the patient exposed to the danger of an almost certain recurrence, sooner or later followed by death—that the extirpation of an epithelial or fibro-plastic tumour is much more hopeful and satisfactory in its results? If, in the latter case, a return of the disease should take place, the surgeon may operate again with greater hope and courage, knowing well that a radical cure is not impossible.

Let us, then, study these diseases more and more, and let us endeavour to distinguish them from those maladies with which they are often confounded. Let us strive to learn still more accurately their clinical differences; and, in doubtful cases, let us always avail ourselves of the aid afforded us by the microscope.

M. Leblanc said that he did not consider, with the microscopists, that cancer did not exist unless the heteromorphous elements, called cancer-cells, were found in the morbid growth. He gave a more extended signification to the term, and included under it all those grave lesions which never healed spontaneously, which became generalized, and which possessed physical characters peculiar to each species of tissue. To determine these characters with certainty, the microscope should, in all cases, be used. Among the lesions which he considered cancerous, were a large number which consisted of the substitution of a homeomorphous, but abnormally placed element, for some other homeomorphous element which was absent at the diseased spot. This was what occurred in epithelial and fibro-plastic growths. They were anomalous accumulations of normal elements occurring, in different proportions, in regions where they ought not to be found in a state of health. The union or simultaneous occurrence of fibro-plastic, epithelial, and melanotic tumours, was by no means uncommon. He cited an interesting case, which proved that fibro-plastic tumours may return, either at the same or at a different place; that these tumours may coexist in the same individual with other growths demonstrated by the microscope to be true cancers; and that each of these varieties possesses a different degree of importance. In every case it was futile to attempt to form a correct diagnosis or prognosis by clinical observation alone, without also appealing to the microscope.

Veterinary pathology presented many good cases illustrating the curability of cancer; and he could advance many instances in which a radical cure was obtained by operating *early* on animals, while the disease was as yet local and not fully developed. It was the same in human as in veterinary medicine—the chances of recurrence were materially lessened by an early operation.

M. Barth exhibited to the Academy a tumour which had been removed, by M. Amussat, jun., from the testis of a boy, æt. 11, several of whose relatives had died of cancerous affections. The tumour had all the appearances of encephaloid cancer, save that no juice could be obtained from it by scraping or compression. One of the most distinguished microscopists had been unable to find any cancer-cells in it; while another gentleman, equally expert in histological investigations, had demonstrated their presence. M. Barth himself had seen none; and he therefore regarded the growth as a fibro-plastic tumour of the worst species. It seemed to him probable that the hereditary cancerous taint had, in this boy's case, given a malignant character to what was, naturally, a simple and benign tumour. M. Barth discussed the relative value of the microscopical characteristics of cancer, and remarked, in conclusion, that although the microscope had not revealed cancer-cells in this tumour, he did not the less consider it to be a malignant growth. He was quite convinced of the utility of the microscope, but thought we should guard against over-enthusiasm for it,

which would lead us to exaggerate its value, and be deceived by its applications.

M. Velpeau exhibited several tumours which he had recently removed, the nature of which he had diagnosed before their extirpation. "I only exhibit these specimens," said he, "to show that clinical diagnosis is not uncertain, and that, when I affirm having seen a tumour, I wish my word to be accepted."

M. Gerdy—(with great warmth).—I cannot allow this. I protest strongly against such a pretension (*une semblable pretention*).—(Great excitement).

October 31.—M. Larrey did not intend to have taken part in the discussion, but he was surprised to hear so much said, both for and against the microscope, without the cause of the instrument being defended by any of those who had made its use a special study. The microscopists had hitherto abstained from speaking, and it had required all M. Robert's talents to defend the instrument from the imputations cast upon it by M. Velpeau. He possessed no claim to constitute him an efficient partisan of the microscope, but he thought we might as well deny the utility of pathological anatomy as of microscopical observation.

If the cancer-cell appeared, to microscopists, the distinctive cancerous element, we ought not to deny that it was so, in spite of certain exceptional cases, and those differences of opinion which were more apparent than real. One objection urged against the use of the microscope was that its employment required an amount of patient investigation which was incompatible with the exigencies of the profession, and with the acquirements of most medical practitioners. But it was a pity that we should neglect a thing which was really useful, merely because all of us were unable to use it properly. He himself, in doubtful cases, always referred the matter to his microscopical *confières*. It had been said by others that we had presumed too much upon the value of microscopical appearances. The discrepancies of opinion among microscopists were to be explained in two ways: 1st, They certainly were not infallible in their dicta; and 2d, the mistakes of the inexperienced histologists had brought discredit on the more able observers. Another reason of their diversities of opinion was, that observers were seldom furnished with *entire* tumours for examination, but merely with fragments of them. Now, if these tumours were of a mixed character, they might easily contain cancerous elements in certain parts, and none in others; a circumstance which would occasion contradictory results, if they were examined piecemeal. Moreover, it was not fair for surgeons to try to puzzle microscopists by submitting to them morbid structures for examination, without previously informing them anything about their history, course, diagnosis, or other elucidatory circumstances.

As to the curability of cancer, the microscope—although it was not an infallible test—had not been consulted in the majority of the so-called cases of true cancer, said to be radically cured; hence he entertained strong doubts as to whether cancer was ever curable. One of the most renowned practitioners they had ever known, Marjolin, did not believe in its absolute curability; he looked upon every truly cancerous affection as sooner or later fatal by relapse, and believed a radical cure only to be possible in those tumours which were benign, or had been erroneously diagnosed. Could not this distinction, which Marjolin established from practical observations only, be decided with habitual precision by the aid of the microscope? The real value of this instrument consisted in the aid it afforded us in elucidating cases of doubtful, and rectifying those of erroneous diagnosis. M. Larrey cited the details of several interesting cases in which benign fibrous tumours had been mistaken for cancerous, and showed the advantages of microscopical examination in such cases.

In conclusion, he considered that the Academy was not as yet in a position to decide definitely either for or against the utility of the microscope in forming a diagnosis and prognosis of cancer. He proposed that the Academy should hear the cause of the instrument defended before it, by one or more of the microscopists.

M. Malgaigne considered that the microscope had been too much depreciated by M. Velpeau, and too much praised by M. Robert. He thought that the microscope had been of little real service to surgery. He went over, in a rambling manner, the clinical views of cancer which had been held by the older

surgeons, as Boyer, Scarpa, Bayle, Laënnec, Astley Cooper, and Abernethy. He said that the microscope had made a complete revolution in surgery; he did not admire the instrument, and still less those who used it. If the microscopists had not been kept in check by surgeons, they would have destroyed science altogether.

He thought that there were three kinds of cancer; namely, that containing the characteristic cells; epidermic cancer, and fibro-plastic tumours; and all of these were malignant. M. Velpeau had been very severe on the microscope, yet but for it his work on *Diseases of the Breast* could never have been written, for we there find tumours divided into malignant and benign—true cancers and fibro-plastic growths. M. Velpeau had said that he could dispense with the microscope. *He* might be able to do so, but M. Malgaigne affirmed that, for the bulk of practitioners, *a scientific diagnosis was impossible without the employment of the microscope.* Those who obstinately refused to employ the microscope appeared to him to resemble a person with imperfect sight, who would not use spectacles, affirming that he could see just as well without them. The question of the curability of cancer was entirely one of correct diagnosis, which could never be made without the microscope.—*Monthly Journ. Med. Sci.*, Dec. 1854.

[We shall give the continuation of this protracted discussion in our next No.]

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35. *Cancer.*—Mr. I. Z. Laurence read (Jan. 10, 1855), before the North London Medical Society, a paper on cancer, of which the following is a short abstract:—

According to Mr. L., cancer is "a growth which is obviously but the local manifestation of a diathesis, the tendency of which is to destroy life, either in virtue of its own local effects, or of that diathesis, or of both conjoined." Cancroid differs from cancer but anatomically, in the comparative rarity of secondary deposits, and in its minute structure. Semi-malignant forms of disease are morbid products, as it were, locally malignant, but constitutionally innocent. The diagnostic data of a surgical tumour were divided into two groups: 1. The clinical; 2. The anatomical. The former were considered by the author *seriatim* at some length. In reviewing the physical properties of tumours, Mr. Laurence insisted strongly on the advisability of employing the grooved needle previous to any operative procedure in all cases that were at all of a dubious character. He related a case of submaxillary chronic abscess, in which the utility of this practice was forcibly illustrated. Mr. Laurence considers the so-called cancerous "cachexia" of indisputable occurrence, but of little diagnostic value. The anatomical properties of tumours were now passed in review. Special allusion was made to the anomalous characters cancerous tumours often assume, when they originate in the osseous tissues, and illustrated by several cases that had been examined by the author. The relations, the clinical phenomena attending the development of morbid growths in the organism were now discussed in considerable detail. Mr. Laurence gave as the *resumé* of his researches on the minute structure of tumours the following conclusions: "1. That, in the greater number of cancerous tumours, the 'cancer-cell' will be found to exist. 2. That this cell-form occurs now and then in growths clinically innocent. 3. That, *vice versâ* (what is, however, more rare), tumours anatomically innocent prove clinically malignant. That the cancer-cell is not the *sine qua non* character of cancer.—(Velpeau). 4. That the results afforded by the microscope must take an important but not exclusive position in the series of diagnostic data."—*Med. Times and Gaz.*, Feb. 10, 1855.

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36. *Cancer successfully Treated by External Application of Caustic.*—Dr. J. D. Gillespie related the following case to the Medico-Chirurgical Society of Edinburgh:—

A gentleman, aged seventy-nine years, of a large robust frame, in 1847 received a slight abrasion of the heel, which speedily cicatrized, but the cicatrix soon after gave way and ulcerated. The ulcer dried up and left a hard warty substance in its place, which continued to increase. In 1851, it had assumed the appearance of a hard cancerous growth, fully half an inch in depth. By the advice

of Mr. Syme, caustic was applied. Erythema of the leg soon after set in and reached as far as the groin, and the tumour increased in size. A second attack of erythema occurred a few weeks after. The tumour continued to increase in size; the glands in the groin became enlarged; the general health began to be impaired, and the mental faculties were affected. It was in October, 1851, that caustic had been first applied; the tumour was then the size of a walnut, spongy, and occasionally bleeding. Chloride of zinc was had recourse to, and the tumour came away, but after cicatrization had commenced, the tumour reappeared. The application was continued for a year with similar results, repeated hemorrhages took place, and the glands in the groin were permanently enlarged. Potassa fusa was applied on three different occasions, but the chloride of zinc was decidedly more efficacious. On September 18, a year after the first application, chloride of zinc was tried for the twentieth time, and the tumour to the extent of three inches was destroyed; a part of the os calcis exfoliated. Two years have now elapsed, and there had been no tendency to reproduction. Dr. Gillespie considered the case interesting, as it was just such a one which was occasionally vaunted by some quack as a victory gained over modern surgery, and this one certainly explained their so-called cures, and at the same time it was fraught with caution to the surgeon not to lose an opportunity of giving similar cases a trial with the caustic before dooming them irrevocably to the knife.

Professor Miller referred to cases of soft cancer of the breast where a cure was sometimes obtained by the diseased product sloughing *en masse*. He could corroborate the statement of Dr. Gillespie with reference to the efficacy of the chloride of zinc; as a caustic it was thorough, and easily kept under command. —*Monthly Journal of Medicine*, January, 1855.

37. *Malignant Disease of the Ilium.*—Mr. Fergusson, in a clinical lecture delivered at King's College Hospital, gave the following account of a case of this kind.

"T. B., aged 56, a bookseller at Birmingham, states that he has always been very healthy, although subject latterly to great debility. About last Christmas—that is, nearly four months ago—he began to notice a pain in the posterior part of the left haunch bone, which was at first trifling, though increased by the slightest exercise, and always relieved by rest. The pain gradually increased in severity until six weeks since, when it came on rather more sharply than usual, and then it was that he noticed a swelling in the situation of the pain. This swelling was then as large as the top of his thumb; and the pain, when it commenced, was, according to his feelings, situated in it. He moreover states that, when he first discovered the tumour, he found it pulsate distinctly. From its appearance to the present time the tumour has been steadily and slowly increasing. Present condition: The patient is in a state of great debility, which has been slowly coming on for the last two years. Bowels much confined; appetite good; sleeps well; pulse 90 and weak. There is a tumour situated on the posterior part of the left dorsum ilii, above and slightly over the great sacro-sciatic foramen. The crest of the ilium can be felt distinctly above it. The dimensions are, five inches in one direction, and four in the opposite; its circumference is nearly circular, and it stands out from the surface of the body about one inch. When the fingers are placed upon the tumour, it may be distinctly felt to pulsate, each pulsation being synchronous with one at the wrist. If the stethoscope be applied to it, a blowing sound in successive puffs is plainly audible. It is not tender to the touch. The skin over it is freely movable, and not discoloured. A bladder filled with ice is kept for two or three consecutive hours over the tumour, till the skin becomes quite numbed. This has a very decided effect on the tumour, causing it to become hard, with the pulsation diminished."

From the history, I think you would naturally suppose the case to be one of aneurism of the gluteal artery; and for such it was taken by the surgeons who sent the patient to me from Birmingham; indeed, so certain were they, that it was considered a proper instance for ligature of the internal iliac artery. I

have never seen an aneurism of the gluteal artery, and I believe spontaneous aneurism of that artery to be extremely rare.

After the admittance of the patient into the Hospital, I made a careful examination; and this is what struck me: Here was a man lying in bed, with a large pulsating tumour, resembling in many ways an ordinary aneurism; pallid, thin in the limbs, hardly able to walk, and experiencing great pain in and about the region of the tumour. Now, contrast this with a case of spontaneous aneurism of the popliteal or other large artery of the extremities, but especially with one situated within one of the large cavities. The patient labouring under the disease is much annoyed at being kept to his bed; he imagines that little is the matter with him, for he may be capable of undergoing great fatigue, perchance walking great distances to seek opinions regarding his malady. In fact, it is not a feature in ordinary aneurismal disease for the patient to be out of health. I mentioned that great pain was very early complained of in the tumour, whereas it is rare for those labouring under an aneurism of a large artery to make such a complaint at so early a period.

Regarding the character of the tumour: Where I made pressure in the site of the gluteal artery, as it leaves the sciatic notch, and turns upon the outside of the pelvis, all pulsation in the tumour ceased, which fact certainly seemed very strong in favour of the supposition of a gluteal aneurism, and an indication which, taken by itself, was not unlikely greatly to bias the surgeon in his diagnosis; but I confess this had little or no weight with me, for I have met with malignant tumours of bone in various parts of the body possessing great pulsation, which could be arrested by pressure on the main artery supposed to lead to the disease.

The size of the tumour and its situation were very much against it being considered an aneurism of any one branch of the gluteal; indeed, I could not fancy such constitutional disturbance to be dependent upon such a source. It is seldom that the surgeon is consulted about an aneurism at so early a period as this case came under notice; but, if he should meet with such a disease, at what is termed its first stage, it is generally possible to empty the blood out of the tumour; but, as you saw during my visits to the patient, I frequently tried so to do, but was always unsuccessful. In the earlier stage of the disease, the crest of the ilium could be distinctly traced above the upper border of the tumour; but, after a short time, the part became entirely involved in the disease. There was one feature in which commonplace anatomy greatly assisted me. I noticed that the insertions of the long extensor muscles of the back, as the disease increased in its dimensions, became elevated above the surface of the tumour; whereas, if the swelling had been an ordinary aneurism of the gluteal artery, they would doubtless have been beneath. These were, then, the principal points from which I framed my diagnosis, in the early stage of the disease; and I came then to the conclusion, that the man was not the subject of a gluteal aneurism, as was at first supposed, but that he was labouring under malignant disease of the ilium, and that the tumour was what is commonly called a pulsating tumour of bone, possessing more vascularity than was usual in such cases; and hence the well-marked pulsation.

This so-called aneurism of bone is, in my opinion, nothing more than aneurism by anastomosis in hard structure, somewhat modified in its condition and symptoms, and which might in many instances be called medullary sarcoma, or fungus hæmatodes. There is generally a very large expansion of bone, seeming as if the bone itself had been blown up, the interior of which may contain a quantity of medullary or melanotic matter, or resemble a sarcomatous mass, containing cysts with fluid blood, and possessing great vascularity. Though thoroughly satisfied in my own mind as to the nature of the disease, I was anxious, nevertheless, to avail myself of the advantage of other opinions; and, accordingly, after requesting the opinions of my colleagues, I invited surgeons of the highest eminence in London to see the case, the majority coming to the same conclusion that I have already expressed.

It was quite evident that an operation could in no way benefit the patient, and that the only treatment that could avail was rest and kind attention to his wants. He daily grew weaker, until at length, becoming delirious, he ultimately

died from pure exhaustion. Here let me impress upon you how important it is to avoid all surgical interference in those instances in which such a proceeding is unlikely in any way to better the patient, but which, on the other hand, is calculated to bring discredit on the surgeon, and the worst possible result to the sufferer. This disease so nearly resembles gluteal aneurism that surgeons of experience have actually mistaken its true nature, and treated the case as if it really had been an ordinary aneurism. Among such instances I may mention two cases which occurred to Mr. Guthrie and Mr. Stanley, in which the main artery was ligatured; but subsequent results showed the error that had been committed. These gentlemen have candidly laid the whole proceedings before the profession, and therefore I have no hesitation in mentioning their names.

By studying these unfortunate cases, which are justly held out to us as landmarks, we are enabled to avoid falling into similar errors; and I often think that in our profession as much is learned from mistakes as from the performance of good surgery.

Some time before the patient's death, while paying my visit one day, I found that the left thigh-bone had spontaneously broken just below the trochanter, no cause being assigned for the accident. Now, in malignant affections, or certain forms of cancer, it is not by any means uncommon for bones to become friable, and break with the least possible exertion. This was, therefore, another very conclusive proof in support of the malignant nature of the disease.

These were, then, the principal points of interest in this remarkable case; and the *post-mortem* examination proved the correctness of the leading opinions that had been formed during life. Most of you saw that examination; but I will briefly detail the morbid appearances which were presented. When the body was put on the table, you saw the extreme emaciation to which the patient had been reduced by the malignant tendency of the disease. On reflecting the skin from the left buttock, and dissecting back the gluteus maximus, which was greatly wasted, a large tumour was found, reaching from the sciatic notch to the brim of the pelvis—in fact, covering the whole of the haunch bone, and resembling more the substance of brain than any I remember to have seen. The tumour, I think, might with propriety be called by half-a-dozen different names, each being equally applicable, as medullary sarcoma, encephaloid disease, soft cancer, etc.

The disease had evidently commenced in the cancellated structure of the ilium, and found its way into the cavity of the pelvis, as well as outwardly. That part projecting into the pelvis had elevated the iliac vessels, and put upon the stretch the large nerves of the sacral plexus, which fully accounted for the constant pain complained of in the thigh, extending to the foot. That portion of the ilium in which the disease commenced was entirely destroyed, the free margin of the bone being left rugged and bare.

The gluteal artery, much enlarged, could be traced, shortly after leaving the sciatic notch, winding over the lower part of the tumour, and distributing large branches, which ramified freely over the surface, and fully explained the reason of the well-marked pulsation during life. The large sacro-sciatic nerve was pressed upon by the lower part of the tumour, which doubtless caused the excessive and frequent pain to the patient. The disease had extended to the left side of the two lower lumbar vertebræ, sacrum, crest of the ilium, and upper part of the femur. The remaining bones of the body were not found similarly affected, but the kidneys and lungs presented well-marked specimens of cancerous deposits.—*Med. Times and Gaz.*, Dec. 9th, 1854.

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38. *Treatment of Aneurism by Compression.*—Mr. Fergusson, in a recent clinical lecture, delivered at King's College Hospital, made the following interesting remarks on this subject:—

"From the time of the celebrated John Hunter down to the present day, aneurism, especially of the popliteal artery, has been deeply studied, and, in attracting such an amount of attention, has led, I believe, to a more accurate knowledge of external or surgical anatomy, which renders the surgeon of the

present day fully competent to perform a most difficult and hazardous operation for the cure of this formidable disease.

"Since Hunter first tied the femoral artery for the radical cure of a popliteal aneurism—now seventy years ago—a vast deal has been done in this department of surgery, for the Hunterian operation has been made applicable to the radical cure of aneurism of many of the large arteries in various parts of the body. The carotids, iliacs, subclavians, &c., have been tied with signal success; and a ligature has even been placed around the abdominal aorta and innominate artery, but, unfortunately, with fatal results.

"The Hunterian treatment by ligature proving so successful the operation obtained the sanction of the profession, and young surgeons, anxious to distinguish themselves, were not slow in imitating the example; and it so happened that, if they were successful in the treatment of their cases, they obtained a well-merited *éclat*, which adds a stimulus to exertion, and always meets with its reward.

"The Hunterian operation being fully established, all other methods formerly employed had fallen, in a manner, into disuse; but, when it was shown that men had courage to countenance any new method that presented, there was then a return to plans that were in existence prior to the time of Hunter. It was also strange that one at least of these methods was esteemed much more worthy of attention than was at first supposed. The plan that I allude to is compression of the main artery leading to the tumour. No doubt this plan is far less brilliant in its details than a daring cutting operation; and more credit is due to those who were the means of its revival, when it is remembered that it was resorted to at a time when a successful operation was in continual practice with the most satisfactory results.

"The advantage of compression in the treatment of external aneurism, when practicable, over that by ligature, is at once evident. By inflicting one kind of injury, we are often enabled to get rid of a still greater mischief, and this is considered good surgery; but if, by a process less dangerous and hazardous, and without performing a cutting operation, the same disease or injury can be cured, surely it is a better style of surgery, and one that approaches nearer to the perfection of our art.

"The history of the case is short; it is briefly this: A. B., aged 22, married. Has always enjoyed pretty good health. The first symptom of her disease occurred twelve months ago. This was pain, confined to the popliteal space of the left leg; she could assign no reason for its coming on, as she never had any accident to that leg. The pain, which has lasted to the present time, occurs in paroxysms of variable length, sometimes lasting for a whole day, and at other times not an hour, and accompanied with intervals of perfect ease. The pain is always aggravated by exercise, or by merely extending the leg. It has never been bad enough to prevent her following her ordinary occupation. Soon after its first occurrence, she noticed a hard, throbbing tumour, the size of a hazelnut, in the centre of the ham; it was not tender to the touch, although it seemed to be the seat of the pain. It has remained almost unchanged since its first appearance; the only alteration that has taken place is a trifling increase in its size. It is quite firm and pulsating. Pulsation is completely arrested, if the superficial femoral be compressed. A bruit is heard on applying a stethoscope to the tumour.

"There are one or two very singular features in this case. The first is, the occurrence of a popliteal aneurism in so young a person. You may never again see a similar case. I have never met with one before in so young a subject, and I imagine hardly a similar instance is to be found in the records of English surgery. The chief point, however, to which I wish to direct your attention is the plan of treatment that was pursued. It is one that was in vogue with surgeons two hundred years ago, and various accounts of it are to be found in the works of older authors. After the successful trial of Hunter's operation it was given up; but within the last few years it has been revived by some of the most eminent surgeons, especially those of Dublin, who have taken precedence in this interesting subject. A full account of the practice will be found in some of the established modern works, and a slight sketch in my own book on 'Prac-

tical Surgery.' Since the revival of this method, there has been sufficient success to strike most surgeons with astonishment; for out of a long list of cases treated by compression, success has been great beyond anticipation, and much greater than when the disease had been subjected to the treatment by ligature.

"This method of treatment is now very generally sanctioned by the profession, and I am not aware of any distinguished surgeon, save one, who strongly objects to at least a trial of this plan. I allude to Mr. Syme, who has been remarkably successful in a great number of cases in which he used the ligature; for he states that in tying the superficial femoral for the cure of popliteal aneurism in upwards of twenty cases, he has not met with a fatal result. But I have seen the artery tied equally well by other surgeons of eminence, in well-selected cases, though not with the same beneficial effects. Mr. Syme has expressed his opinion so strongly in favour of the ligature over compression, that he has endeavoured to cast a certain amount of discredit on those who have ably devoted their time and energy to the trial of a more simple and less hazardous cure of this formidable disease. On the revival of compression for the cure of popliteal aneurism, several improvements were made with regard to the manner of applying the pressure, and old notions regarding the duration of time it was necessary to employ were soon eradicated. It was early found that there was no need to apply the pressure so forcibly as to keep the sides of the vessel together, thereby inducing inflammation and subsequent adhesion, which led to the obliteration of the canal; for the operation of Bradsor and Wardrop of placing a ligature on the distal side of the tumour, showed that to obtain a satisfactory result it was not necessary to cut off the entire flow of blood through the tumour; and subsequent experience has proved, that in the treatment by compression it is not requisite so to do, but that gentle and continuous pressure modifying the course of the current is quite sufficient, in the generality of cases, to cause consolidation within the tumour.

"Again, the pressure need not be constantly applied to one particular spot, but may be shifted from place to place along the course of the artery, so as to give the patient as little pain and annoyance as possible.

"The result to be hoped for in the treatment of these cases is, coagulation of blood and deposition of fibrin in the aneurismal tumour, which, as I have said, can be produced in many cases by simply modifying the flow of blood through the main artery. The femoral artery is never obliterated at the point where pressure is applied, but can be traced almost down to the tumour itself. If the case go on favourably, and the patient be a proper subject for the plan of treatment, a cure may take place in a few days, or in a week or two; but if, on the other hand, the patient be not content to put up with the inconvenience and pain for a time, the cure may be protracted for a lengthened period. This was well illustrated in a case I had under my care some time ago, when, owing to the extreme irritability of the skin and the condition of the patient, sloughing occurred in the groin, occasioned by the pressure, which greatly retarded the cure.

"I do not intend to enter more fully into the merits of the several methods for the cure of aneurism, but shall content myself with stating the treatment I employed in the case under notice, and the results which followed. The aneurism belonged to that class which by Scarpa was termed a 'mixed aneurism;' that is, there was probably a dilatation of all the tunics, and then subsequent rupture of the two internal coats.

"I employed the instrument that is most in favour among the Irish surgeons. Carté's 'circular compressor,' in which a very ingenious addition to the common screw force is applied by means of bands of caoutchouc, whereby a certain resiliency is acquired, which keeps up effectual pressure, and at the same time possibly obviates any injurious effects from the screw. Among other contrivances, I may mention the tourniquet of Signoroni; also the one invented by my friend, Mr. Bulley, of Reading, which has two screw pads, allowing the pressure to be applied at different spots at various periods. There are other equally ingenious instruments which possess their individual merits.

"At first, I must confess, I did not anticipate a favourable result, as the patient did not appear to be fully aware of the great danger of the disease, and

was seemingly very loth to submit to the treatment; but, by explaining to her the importance of resolutely bearing the inconvenience attendant upon such a plan, in all probability a more serious operation would be avoided, she accordingly determined to aid us in the cure; and the following sketch of the treatment shows the satisfactory result:—

"I applied the pressure to the common femoral immediately below Poupart's ligament, regulating the force to such a degree as to keep up an amount of pressure just sufficient to stop the pulsation in the tumour. It was continued for twenty hours, when, some uneasiness being complained of, it was omitted for an hour or two, and then reapplied. On the third day the pulsation was not so full or throbbing as before. On the ninth day the pressure was modified to such an extent, sufficient to weaken, but not completely to arrest, the flow of blood into the artery. On the fourteenth day, the woman not feeling at all well, all pressure was removed for a few days. On the nineteenth day it was readjusted, but, in the evening of that day, the pulsation in the tumour became feeble in a marked degree, and some slight pain was complained of in the ham. On the twenty-first day all pulsation had completely ceased, and the tumour was found diminished in size and of considerable hardness. It was now evident that a cure was effected; and no return of the disease has as yet shown itself.

"The most interesting point in the treatment of this case is the rapidity and easiness with which this disease was cured; and it also well illustrates the fact that it is not always absolutely necessary to make continuous pressure on the artery for any length of time."—*Med. Times and Gaz.*, Dec. 30, 1854.

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39. *Aneurism.*—The *Medical Times and Gazette* (January 20, 1855) contains a report of two cases of aneurism of the lower extremity, lately treated at the Liverpool Royal Infirmary. In one case, compression was successfully employed; in the other, the femoral artery was ligatured, which was followed by gangrene and death.

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40. *Fracture of the Pelvis, with Laceration of the Urethra.* By James Syme, Esq.—[The following very interesting cases are related in a clinical lecture, delivered during the winter session of 1854–5]:—

"The next case to which I shall allude is one of great interest, being very serious, and also of considerable rarity. The patient is A. M——, aged thirty-four, pointsman at the Cobbingshaw station of the Caledonian Railway. On Monday, the 13th November, he was crossing the line, at 8 P. M., when an engine, coming upon him unawares, at a speed of about fourteen miles an hour, struck him upon the right side of the pelvis, and knocked him down. He was insensible at the moment, but soon came to himself, and was brought by train to Edinburgh, and admitted into the Infirmary about 10 P. M. At this time there was some swelling of the right thigh and groin, and, on examination of the pubic arch, an abrupt deviation from its natural line was felt about the junction of the rami of the ischium and pubes on the right side, and on pressure with the finger on this spot, very distinct crepitus was perceived, a small portion of bone being apparently detached, and freely movable. As a measure of precaution, an attempt was made to introduce a catheter, but no urine could be drawn off, although the instrument passed pretty freely backwards, and was supposed to have entered the bladder. He was, therefore, put to bed, a flannel bandage being passed round the pelvis. Next morning, however, the patient complained of inability to pass urine, and a tumour began to appear in the hypogastrium. Another attempt was now made to introduce the catheter, but without success, its point having a constant tendency to slip out of the membranous part of the urethra. I was then asked to see the patient, and passed a full-sized instrument, which was tied in and remained for two days. About midnight of the day of its removal, I was again called to the case, in consequence of retention, with distressing symptoms. I again passed the catheter, which was now retained three days and a half, the patient not complaining of any inconvenience from its presence. But, within a few hours after it had been removed the second time, it became again necessary to introduce it, the patient being unable to pass a drop of urine. The instrument has been since retained,

having been now, for the second time, three days in the bladder, in addition to the first two days. It now becomes a question, What is to be done? A man cannot always wear a catheter in his bladder: a metallic instrument is liable to cause injury by producing ulceration, and I believe it would be impossible to pass a flexible one. It would appear that the injury inflicted on the urethra has been such as to produce a valvular condition at the lacerated part; certainly no extravasation of urine has occurred, otherwise death must speedily have resulted from its escape from the part of the canal injured.

"There seems to be something wrong in this case, which it is not in the power of nature to remedy. After careful consideration of the circumstances, I have concluded that the catheter must now be removed, and that if he should be still unable to pass urine, an incision must be made, so as to reduce the injury to the condition of a simple wound. There are two situations where this might be done; either laterally, as in lithotomy, or in the median line. It is not a matter of indifference which of the situations be chosen, that in the middle line being much the safer; for the artery of the bulb, which is a vessel of very considerable size, enters the bulb at its lateral aspect, just at the anterior part of the membranous portion of the urethra. Now, if a knife be entered into the membranous part of the canal at its lateral aspect, and carried forwards into the bulb, then (and engrave what I now say upon your memories till the last day of your existence as practitioners of surgery!) to an absolute certainty, must the artery of the bulb be wounded. Hence the common cause of wound of this vessel in lithotomy is division of the urethra too far forwards, and this is particularly liable to occur to those who use a straight staff. In the present case we shall have, very probably, to cut as far forwards as the bulb. The artery of the bulb is hardly within reach of ligature, and if pressure be applied to repress the hemorrhage, extravasation of urine into the cellular tissue of the pelvis must of necessity occur when the patient makes an effort to pass urine. All this risk will be avoided by cutting in the middle line upon a staff grooved on the convexity, and then introducing a short tube through the wound into the bladder. The short tube is far preferable to a catheter in such a case, for here the incision, unlike that for stricture, involves the deep fascia of the perineum, and, if the beak of the catheter should slip for a short distance, the patient would be liable to fatal extravasation of urine. I speak from sad experience. A healthy young man once applied to me with a stone in the prostatic part of the urethra. I extracted it easily, introduced a catheter, and tied it in. Next morning, however, I found him dying; the catheter had slipped, and urine had become extravasated into the pelvis.

["The patient continued unable to pass urine after the removal of the catheter, which was again introduced by Mr. Syme in the night. He found that the parts in the vicinity of the injury of the urethra were in a less satisfactory state than before; they were becoming softened, and a cavity containing urine had formed around the seat of laceration. Mr. Syme, therefore, resolved to proceed to the operation to which he had alluded in his lecture; and accordingly the next day (Nov. 24th), the patient being under the influence of chloroform, he introduced into the bladder a staff grooved on the convexity, made an incision in the middle line of the perineum, and, having introduced the point of the knife into the staff, pushed it along so as to divide freely the membranous part of the urethra, which he could distinctly feel to be lacerated; a short tube, such as is used after lithotomy, was then passed into the bladder through the wound, and tied in. Not more than an ounce of blood was lost in all, including subsequent oozing.

"Mr. Syme, in remarking on the operation, said, that the only source of regret was that it had not been performed earlier. The prognosis was now rendered very unfavourable by the jaundice which had come on during the last twenty-four hours, and by a livid discoloration in the perineum, which looked very like sloughing of that part. The patient felt at first much relieved after the operation; but, on the following day, he was seized with very severe pain in the hepatic region, and acute tenderness below the right costal margin. Leeches were applied, and calomel and opium administered; and, under this treatment, the pain and tenderness subsided, and in a few days the jaundice

disappeared. Healthy lymph upon the surface of the wound showed also that the suspicion of sloughing of the perineum was groundless; and the prognosis became, on the whole, much improved. The almost total loss of appetite, however, which he had before the operation, and during the hepatic attack, produced great emaciation; and this, together with his lying always on his back, in consequence of the fracture of the pelvis, while the bed was more or less wet with urine, induced extensive bed sores, which have since been the chief difficulty to contend with in this case.

"The tube was removed forty-eight hours after the operation. The urine then came freely away, and passed entirely by the wound for a fortnight. Mr. Syme then passed a full-sized bougie, which went smoothly into the bladder; after which the patient passed a few drops of urine by the penis. During the following month the amount of urine passed by the natural passage increased, the bougie being introduced at intervals of about ten days. The use of the bougies has not been continued since; and a considerable proportion of the urine is still (February 6th) passed by the fistulous opening in the perineum; hence the difficulty of healing the bed sores continues to be great, and he is prevented from making the progress which he doubtless would otherwise make. Still, by the use of a large water-pillow, and frequent change of bed-clothes, the back is now almost healed, and his general strength is on the increase, so that he is able to support himself in bed, during the dressing of the back, more than ever before.

"On the 22d of January, Mr. Syme showed to the class the urethra and parts of the pelvis of a young man eight years of age, who had fallen from the roof of a lofty house, and died in the hospital a few hours after. On admission he was almost unconscious, and moving his limbs about in a wild manner, and these symptoms, together with a severe bruise on the right side of the head, led to the suspicion of laceration of the brain, which was found to exist on *post-mortem* examination. A few drops of blood from the penis led to a careful examination of the pelvis, when a comminuted fracture of the descending ramus of the right os pubis was detected, and a catheter was kept in the bladder till a little before his death. On examination of the body, it was found that the horizontal and descending rami of both pubic bones were broken; and there was a comminuted fracture of the right side of the sacrum, through the line of the foramina for the passage of the nerves. Very extensive ecchymosis existed about the fractures, and the membranous part of the urethra was completely separated from the surrounding parts by coagulated blood, while some of the fragments of the pubes lay very close to the canal. On laying open the urethra, however, on a full-sized bougie, it was found to be quite entire, some bruising of the membranous part, and of the spongy portion near the bulb, being the only injuries it had sustained. Mr. Syme, after pointing out these facts, spoke as follows:—]

"You recollect a patient who is still in the hospital, to whose case I directed your attention in a former lecture, who had suffered fracture of the pelvis, and laceration of the membranous part of the urethra, and in whom I found it necessary to make an opening into the perineum, on account of protracted retention of urine. In that man's case, the injury sustained by the urethra was only a degree further than what you see here; there the urethra was lacerated at its membranous part, by the fragments of the pelvis; here it is only bruised. You see, from the condition of the parts here present, how necessary was the precaution of introducing a catheter in that case. You observe the membranous part of the urethra is lying insulated from surrounding parts in a cavity filled with effused blood, and had the urethra been torn in this situation, and no catheter introduced, extensive extravasation of urine must inevitably have occurred. After bruising or laceration of the urethra, it is very necessary to introduce bougies occasionally to guard against the occurrence of obstinate contraction of the canal at the seat of injury.

"This has been well illustrated by another case still in the hospital. You remember my speaking to you, in a former lecture, of a miner from Bo'ness, who, having occasion to descend a shaft eighty-four feet deep, adopted the dangerous method of slipping down a rope. He had not gone far, however, when he found

that the rope was wet and very slippery, but he still kept his hold, and shot down with great velocity, lighting on the perineum, upon the edge of a wooden cage used for drawing up the mineral. He then suffered from retention of urine, which the medical men of the district were unable to relieve, and he was, therefore, sent to this hospital. His bladder was then greatly distended, and the perineum was bruised near the scrotum. A full-sized catheter was, however, introduced without much difficulty, although the urethra was obviously injured, and a large quantity of clear urine was drawn off. After a time, bougies were passed with the object of preventing contraction, but not until it had occurred to a small extent; but now, the use of the instruments having been discontinued for ten days, the stricture has increased so much that it will admit only a No. 1 bougie. This same patient, on his admission, complained of severe pain in the back, extending round the waist, and into the legs; and, on examination of the spine, a marked unnatural convexity was found on the lower part of the dorsal region, which was very tender when touched, and surrounded by ecchymosis. There could be no doubt that, the patient having lighted on his seat, the back had been violently bent, and fracture of the vertebræ had occurred, the bodies being compressed, while the spines were separated from one another at the part. The patient, however, has had no paralysis of the lower limbs, and the pain of which he at first complained has now almost entirely subsided. The spinal canal is of much greater capacity than the cord; and this accounts for the fact, that fracture of the vertebræ may occur without any injury to the cord. I have seen repeated instances of this in the hospital; and the present case is a good example of it."—*Lancet*, Feb. 17, 1855.

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41. *Fracture of the Head of the Astragalus.*—M. Vollmar relates (*Zeitsch. f. Chirurg. und Geburtsh.*, iii. 1854) the following example of this rare accident:—A stout countryman fell from the height of eight or nine feet. After the removal of the boot from the left foot, there was noticed a bony prominence in the arch of the foot; and the patient, imagining there must be a fracture, applied cold lotions to the part to reduce the swelling. The foot was found in its natural position, with moderate flexion. In front of the articular extremities of the tibia and fibula there lay, under the raised integument, a bony swelling, separated by a deep depression from the outer malleolus. No hollow, from which the bony mass had escaped, could be detected; the tarsal bones seemed in their normal relations. Extension effected the reposition of the displaced parts, and in four weeks the patient was able to go about, proper bandages having been maintained to keep the separated portions of the astragalus together.—*Med. Times and Gaz.*, Jan. 27, 1855.

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42. *Carbuncle.*—The number of the *Medical Times and Gazette* for December 2d last, contains a report on this disease, with a tabular view of thirty-five cases. The following are the conclusions drawn from the facts there collected:—

1. The frequency of carbuncle has vastly increased of late years, and still continues to do so.
2. But little is known respecting its predisposing causes.
3. It may affect any age, excepting perhaps the very young.
4. Men are much more liable to it than women.
5. It occurs without distinction, at all periods of the year.
6. It occurs in almost equal proportions among the temperate and well-fed and the intemperate and ill-fed.
7. It has a premonitory stage in a considerable proportion of instances.
8. Its general treatment should be by purgatives and alteratives in all cases, and by stimulants or salines, according to the character of the constitutional disturbance.
9. Incisions are demanded when a carbuncle is spreading, or attended by much pain.
10. In a great majority of cases, free incisions relieve the pain, and in a considerable degree arrest also the spread of the disease.
11. If the spreading and the pain have already ceased, no benefit will be

derived from incisions, but the sloughing and suppuration will be much increased.

12. The "core" which separates consists, for the most part, of dead areolar tissue.

13. No proof exists that carbuncle exerts any eliminative influence on the system.

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43. *Treatment of Hospital Phagedæna.*—The prevalence of phagedæna, which, during the past nine months, has been pretty general in the London Hospitals, seems now to be steadily diminishing. It has been very difficult to arrive at any satisfactory conclusions respecting the laws regulating its occurrence. At one period, it has appeared to spread through a certain ward, or to prevail in a certain Hospital, as if by contagion; while at others, observations have been made tending strongly to support the opinion, that it was largely under the influence of atmospheric changes. It has prevailed very irregularly in the different Hospitals, being now epidemic in one, and then, after the lapse of a short time, appearing in another. On the whole, it has been a mild form of the disease. Very few have, we believe, died of it, and a vast majority have recovered after a short and not very destructive attack. In several instances, however, in which stumps, after amputation, have been attacked by it, so much of the soft parts have been destroyed, that a second removal of the bones has become necessary. With regard to the treatment of the disease, the following recommendations might, we think, be summed up as the results of the combined experience of the surgeons who have been most engaged with it:—

1. *As soon as a wound shows a tendency to become sloughy or phagedænic, to have the patient change his bed, and, if possible, his ward.*—This practice was pursued in almost all cases in Guy's and in the London Hospital, and more or less in most others. Often very sudden benefit was remarked. The recommendation, of course, proceeds on the supposed desirability of removing the patient away from any local influences, contagious or endemic, which may have had part in producing the disease. The following case may be quoted as illustrative: A boy, in excellent health, submitted to primary amputation of the arm, on account of a crush, under the care of Mr. Wordsworth, in the London Hospital. On the day following the operation he was remarkably well, and had not the least of constitutional disturbance. During the next six days he continued well, and the stump was granulating healthily, when it became necessary to change his bed, and to put him into one from which a man who had died of pyæmia had been removed. Mr. Wordsworth directed that all the bed furniture should, as a measure of precaution, be removed; and, with the exception of the mattress, this order was complied with. On the morning following the change of bed, the lad was feverish and restless, and his stump had lost its granulations and presented a sloughy surface. He was at once ordered back to his original bed. The phagedæna did not spread; but, almost immediately after the second change, the condition of the stump began to improve, and ever afterwards the advance was uninterrupted.

2. *To destroy fetor by the employment of charcoal.*—In this way, probably, not only is a gas decomposed which was likely to have acted prejudicially on the animal functions, but one which might not improbably have been the means of direct infection.

3. *To employ nitric acid as an application to the sore.*—Most surgeons have formed a very high opinion of the value of this remedy when efficiently used. The acid should be concentrated and pure, and should be liberally applied. We have already at such length spoken in its praise that anything further need scarcely be here added. (See *Medical Times and Gazette* for January 6, page 9.)

4. *To employ as constitutional measures tonics and general stimulants, with in some cases opium, or the chlorate of potash as specific remedies.*—Respecting the use of the latter, a considerable difference of opinion prevails; but instances have occurred in some hospitals which appeared to show almost incontestably their potency in at least some individual cases. The chlorate of potash well deserves a much more thorough investigation as to its remedial powers than it has yet received.—*Med. Times and Gaz.*, Feb. 17, 1855.

44. *Injection of Iodine in Ovarian Dropsy.*—Dr. Simpson mentioned, at a meeting of the Medico-Chirurg. Soc. of Edinburgh (Nov. 15), that he had injected a case in his wards a few days ago, from which he had removed 200 ounces of fluid, and that he had tested the urine for iodine, but without success. It was usually present, however, after the injection of a hydrocele. He (Dr. S.) had now injected some 10 or 12 cases of ovarian dropsy; the disease had recurred in a few of the cases, but in the others, the cure had as yet been permanent. The operation was unattended with any bad results. Indeed, the pulse never rose, and little or no uneasiness was expressed, and that only (if present) in drawing the fluid, apparently the result of adhesions. He employed the Edinburgh tincture of iodine, which had also been recommended for its strength by Mr. Martin, in his paper on the cure of hydrocele by injection.—*Monthly Journ. Med. Sci.*, Dec., 1854.

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45. *Radical cure of Hernia by injections of Iodine.*—M. Jobert lately presented to the Academy of Medicine three cases of inguinal hernia in which this new mode of treatment had been productive of the best results.

The first case was that of a young man, 18 years old, with complete inguinal hernia of the left side, which, though it did not interfere with his ordinary avocations, rendered him ineligible for the military service. The tumour did not descend to the bottom of the scrotum, and it was separated from the testis by a constricting band. It was easily reducible by taxis, but reappeared upon the patient coughing or assuming the erect posture.

At the patient's request M. Jobert performed the following operation on the 12th May. An incision, 2 centim. in length, having been made over the inguinal canal, a fine trocar was introduced into the sac, through which 5 grammes of pure tincture of iodine were injected. The wound was then closed by means of twisted sutures, and dressed with simple ointment. The patient suffered almost no pain. In the evening there was slight tumefaction and pain of the inguinal region, but no fever or constitutional disturbance.

May 14.—The swelling and redness were considerable. The sutures were removed, and the wound was found imperfectly healed.

In a few days the external wound cicatrized, and the swelling and redness disappeared. The left testicle was not atrophied. On 5th June the patient was able to go about, and to cough without the least inconvenience or reappearance of the tumour. As a precautionary measure he continued to wear a suspensory bandage for some time.

The second case was that of a patient, æt. 34, who entered the Hôtel Dieu on the 18th Nov., 1853, having not only a congenital inguinal hernia, but also a hydrocele of the same side. These were separated from each other by an annular constriction of the tunica vaginalis, which contained the protruded bowel above, and the testicle and sermo fluid below. M. Jobert resolved to obliterate the tunica vaginalis, for the purpose of curing both the hydrocele and the hernia at the same time. He accordingly passed a trocar obliquely into the lower portion of the constricted sac, and evacuated the serosity therein contained; and then, having previously made pressure with the finger over the inguinal canal to prevent the possibility of there being any communication between the tunica vaginalis and the peritoneum, he injected some tincture of iodine into the tunica vaginalis and the hernial sac. The external wound was finally closed by adhesive plaster. Next day the scrotum was reddened, somewhat tender to touch, and distended with fluid effused from the tunica vaginalis; but there was no pain or constitutional disturbance. On the 5th Dec., the swelling, which had been gradually subsiding, had nearly disappeared; and on the 12th the scrotum had regained its normal size. The scrotum and the spermatic cord on the affected side were firmer than before; and a cylindrical cord extended along the whole of the inguinal canal, as well as along the spermatic vessels down to the testicle. The patient was completely cured, and he left the hospital able to walk easily and without fatigue, and without any reappearance of the hernial tumour.

The third case occurred in a young man, aged 27, who had had a large oblique inguinal hernia of the left side for eight years, for which he had worn all sorts

of trusses without benefit. At the desire of the patient, and after a consultation with his colleagues, MM. Rayer, Begin, and Cloquet, M. Jobert punctured the hernial sac, and injected the tincture of iodine into the tunica vaginalis. The patient was laid in the horizontal posture after the operation; his legs were flexed on his thighs, and maintained in this position by means of a bolster. The hernial sac swelled immediately after the injection, and in fifteen hours it was as large as the hernial tumour, and had a fluctuating, half-solid feeling. The parts continued thus for eight days, and then the swelling speedily diminished. On the twenty-eighth day after the operation the cure was complete; the track of the spermatic cord was occupied by a hard and compact cylindrical substance, which completely prevented any return of the hernial protrusion, and allowed the patient to go about without any recurrence of his malady. Four months had elapsed at the time when M. Jobert related his case, and the cure was still as complete as at first; the inguinal canal was filled with a hard, solid cord; and there was no atrophy of the testicle.

Remarks.—When the sac is adherent to the surrounding parts—when the hernia is congenital—when the sac is distended with fluid, or is thickened or cartilaginous—M. Jobert recommends puncturing the sac at once with a trocar, without previously making an incision. The trocar is to be inserted perpendicularly at first, then obliquely upwards, and, when it has reached a certain depth, it is to be directed cautiously backwards so as to pucture the sac—the accomplishment of which process is known by the operator no longer meeting with resistance, and by his feeling the canula move about without obstruction in the empty sac. Having entered the sac, the pure tincture of iodine should be injected into it by means of a glass syringe—whereby we may be able to know how much fluid has been used. M. Jobert ordinarily injects one or two spoonfuls (*cuillerées*) of the tincture, but often much less. The quantity injected should always be in proportion to the size of the serous pouch. The injection should be suspended whenever we feel an obstacle to the entrance of the liquid. Instead of allowing the fluid to escape through the canula, M. Jobert withdraws it by means of the suction of the syringe.

When the sac is thin, movable, and easily displaced, Jobert exposes it by dividing the integuments with a bistoury, and *thereafter* punctures it with the trocar—*never with the knife*. In such cases he unites the lips of the external wound by means of twisted sutures.

What occurs after the operation? Immediately after the injected fluid is withdrawn, a new liquid begins to be thrown out within the hernial sac. After twenty-four or forty-eight hours have elapsed, the sac is filled with this fluid; it continues in this state for eight days, and after the first week it begins to diminish gradually and visibly in bulk. In proportion as the tumour diminishes it begins to acquire solidity, and becomes as firm as wood; nay, it eventually becomes as hard as bone or ivory.

M. Jobert affirms that this method by injection is superior to all the other operative procedures which have yet been devised for the radical cure of hernia, inasmuch as it is at once simple and harmless. M. Jobert, in conclusion, acknowledges that this operation is not invented, but merely revived, by him; inasmuch as M. Velpeau, in his *Annales de Chirurgie*, published ten years ago, first suggested the likelihood of injections of iodine proving beneficial for the radical cure of reducible hernia.—*Month. Journ. of Med. Sci.*, Dec., 1854; from *Gazette des Hôpitaux*, August 10, 1854.

OPHTHALMOLOGY.

46. *Employment of Chloroform in Operations on the Eyeball.*—Mr. Haynes Walton, in a clinical lecture delivered at the Central London Ophthalmic Hospital, made the following remarks on the amount of assistance that chloroform is capable of affording to an operator, and the mode in which it may enable

him to obtain a more successful result; how far that assistance is really needed, and when it is judicious to dispense with it.

"Most unquestionably in infancy," he remarked, "chloroform does afford very great assistance; for, without it, resistance on the part of the little patient is certain. You must have witnessed repeatedly the difficulty there is, in certain diseases, in unclosing the eye of an infant so as to obtain a satisfactory view; and surely you cannot forget the little battle that ensues, and the terrible screams that accompany and follow the attempt. The very diminutiveness of the organ, whereby there is much less room for the use of the fingers and for instruments, together with the great delicacy of the parts, demand the utmost exposure of the surface of the eyeball, with the greatest steadiness. In former years, the operation for congenital cataract was frequently postponed, because these desiderata could not be commanded; and I believe that occasionally in the present day their acquisition is considered impossible, and an operation delayed to the great detriment of the patient. I strongly suspect that the reason why the posterior operation for 'solution' has been so frequently advised in infancy, is because of the greater nicety required to perform the anterior one; and you will at once understand, that, without the paralyzing effect of chloroform, to retract the palpebræ, introduce a needle in cornea near the circumference, direct the point in the manner desired against the capsule of the crystalline lens and the lens itself, without touching the iris, or using injurious force, is no easy matter, or, at least, it may not be easy. True it is, that a modern lid retractor, of silver wire, or steel, removes some of the difficulty, but only a part, for the difficulty of securing the child, as well as other obstacles, still remain.

"Again, from what I hear, and indeed from what I see, it would appear that, before the use of the anæsthetic agent, the operation for congenital cataract was not unfrequently left unfinished. The capsule which blocked up the pupil was not always removed; and the operation, in any given case, was more often repeated, not merely from the erroneous idea that then existed, and unfortunately still does exist, about repeating needle operations; but also on account of the difficulty that frequently prevented the surgeon from carrying out his previous intentions.

"With children, and young persons in general, even when an operation is painless, there is an expectation of something worse than what is actually felt, and generally a deficiency of resolution, that renders it impossible for them to be sufficiently quiet without violent resistance, or the employment of mechanical restraint; and failures and mishaps are more commonly due than could be supposed, to the unsteadiness of the eye. If I had noted down all the instances that I have witnessed of foiled endeavours, they would form a large page in my note-book.

"I have alluded to the necessity of mechanical restraint to subdue the resistance of a refractory patient, and this is frequently very distressing to the friends. The public are becoming alive to the disadvantages of the plan, and frequently demand the use of chloroform. A lady called on me recently to make an arrangement for an operation on her child's eye, and at once asked if I used chloroform in such cases. I answered that I did, and, on requesting her reason for the question, she said, 'I have just left the house of Mr. ———, who has declined to employ it; he declared that, by rolling up my child in a tablecloth, and with the aid of two strong assistants, he could manage her, or the most unruly young lady. I told him that her timidity and highly nervous susceptibility made me dread the effect of such an ordeal on her mind.'

"Although there is no necessity to quit my own experience to furnish examples of the difficulty that there may be in operating when opposition is to be encountered, I shall borrow one which occurred to Mr. Wardrop, who had the disadvantage of practising when a calm sleep could not be exchanged for the violent struggles of a refractory patient. The quotation is from Dr. Mackenzie's work. 'A boy fifteen years of age, blind and deaf, at first yielded readily, and allowed himself to be placed and held on the table. The uneasiness, however, occasioned by the pressure necessary to keep the eyeball steady and the lids open seemed to overcome his resolution; and his exertions became so violent,

that it was quite impossible to secure even his head. A second attempt was made the day following, more precautions being taken to secure him; but so violent were his exertions and cries, and so irascible did he become, that all present were glad to relinquish their posts. Some days after, a wooden box, the sides of which moved on hinges, was folded round his body, and fixed by circular ropes, and in this way, notwithstanding a powerful resistance, he was placed on a table and kept quite steady. Much difficulty was found in holding open the lids, and keeping the globe of the eye steady. As soon, however, as the needles touched the eye, he remained quite still, and his screaming ceased.' Mr. Wardrop and others have bled persons till they fainted, in order to obtain a passive state.

"I now pass to those operations on the adult eye, in which we may receive considerable assistance from the insensibility of the patient. It is evident that here there should be a distinction between such cases as require chloroform merely because of lack of moral courage, and those in which it is of positive advantage under any condition; as in the one we may leave the choice to the patient, in the other it is our duty to recommend it; as it is a fact that, with the fullest consent and greatest determination on the part of a patient, indeed, with a resolution that could endure a limb to be severed from the body without a groan or a cry, and with every desire to assist the operator, there are cases in which anæsthetic sleep may be advantageous. The majority of operations for artificial pupil, especially where the proceeding is complicated, and requires the use of more than one, or the re-introduction of the same instrument, falls under the latter category. An eye, for the most part, that requires this aid, is much damaged; the parts with which we have to deal are altered, and the vitreous humour is too frequently disorganized; so that we need the greatest steadiness of the eyeball, with long continuance of a given position, and an absence of much pressure. Now, the movements of the eyeball may be quite involuntary, and the eyelids will twitch, in spite of the most resolute will. But not the least disadvantage of consciousness is the compression that the straight and oblique muscles can and do exercise in such operations. When acting violently, they exert considerable influence; and the effect of such an agency, at such a time, is always hazardous, in several ways. Again, in many operations of general surgery, the sooner the manipulation ends, and the instruments are out of the body, the more certain is the result; this is doubly true of the eyeball. These remarks may be said to apply, in the main, when the eyeball is to be opened for the extraction of any body, be it capsule, animalcule, or any particle driven into it from without, when decided difficulty or intricacy is apprehended.

"I have purposely delayed till now to refer particularly to the formation of an artificial pupil in an infant or a child, because, after what I have said, you may the better appreciate the difficulty attending the execution of such a task, and the better recognize the assistance to be derived from a perfectly passive state. An infant that has lost its pupil from purulent ophthalmia, or any other cause, is not now doomed to darkness till the adult period, or, at least, need not be, as in past years. We can operate on the smallest eye, and the consent of the patient is not necessary.

"The extraction of a cataract may undoubtedly be better performed under the influence of chloroform, both when there is a certain deficiency of moral courage, and when there exist conformations of the eye that render the operation peculiarly difficult, and requiring more than usual dexterity. But as it is after the meridian of life, and often in the very aged, that extraction is needed, we should be careful not to use chloroform needlessly. In the early period of our existence, the risk of a fatal termination is exceedingly small. Not so, however, in the old and enfeebled, in whom the heart is so often diseased by being degenerated—a state which the most rigid scrutiny during life may fail to detect. Disease of the heart, however, has not been always found in those who have been killed by chloroform. Remember, too, that the operation is unattended with pain when well executed. Therefore, you should not use such an agent as a matter of routine, but withhold it when it may be dispensed with; and whenever you intend to employ it, exercise the most searching scru-

tiny as to its admissibility. We should not lightly place an aged person in a state so closely resembling apoplexy, that the most astute physician could not, at the moment, point out the difference.

"Timidity, and the accompanying restlessness of a patient, may render it impossible to operate successfully. I have seen this over and over, even under the hands of operators who have not been surpassed in this kingdom for self-possession and brilliancy in execution. On each occasion, the eye was either lost or much damaged, from the unavoidable results of operating against the patient's resistance. Here chloroform removes all difficulty. I cannot give a case more to the point than the following, which is reported from this Hospital in the 'London Practice of Medicine and Surgery,' in the *Medical Times and Gazette*.

"'A man, 65 years of age, with hard cataract in each eye, was brought up to the operating-room, for extraction to be performed on one of his eyes; but so great was his fright and agitation, that he could not be induced to keep quiet enough to allow his eye to be sufficiently open for the operation. He almost fainted when ascending the stairs, and required to be lifted by the porter. This was from sheer mental emotion, for he was neither feeble nor infirm. As there was no probability of his ever having self-command enough, Mr. Walton determined to give him chloroform. It was decided, however, not to do it on that day, for he had recently taken food, but on the following morning, when his stomach should be empty. This was carried into effect on Friday, and the operation was then easily and successfully performed.'

"It remains for me to mention those physical peculiarities of the eye which impede extraction, and which may be surmounted by the aid of chloroform. They are mainly those that present impediments to exposing and steadying the eyeball sufficiently to enable the cornea to be divided in an ample manner, such as a sunken eye, a narrow palpebral aperture, unusual prominence of the orbital ridge. In any of these states, more pressure with the fingers is generally required than the eye will bear. During stupor, the eyelids can be more widely extended, and the eyeball fixed with a lightness of touch that would, on account of the peculiarities, be insufficient during sensibility; there being, in fact, all the difference between involuntary resistance, however slight, and absolute quiet. Beyond this chloroform does not assist us.

"When a patient has tolerable fortitude, at all events whatever be his mental emotion, so long as he remains master of his will, and can direct his eye to the position desired, and there are not impediments to exposing his eyeball to the required extent, I would rather that he retain his senses during the operation, for then I believe that the crystalline lens is better started from its position, that it escapes more readily, and that the pupil is the more quickly restored to its natural state, and the iris less liable to prolapse after the terrible stretching it has received. Even supposing this not to be the case, and things are equal, is it not far better to save an aged person, if only from all the formality and distress of an inhalation. Is it not better to see a patient rise and walk to his bed or couch, rational and thankful, than for him to be removed, half-conscious, sick, miserable, and requiring careful and anxious attention, both on account of the constitutional effects of the chloroform, and the injury he might inflict on his eye. At a meeting of the Royal Medical and Chirurgical Society, on the 14th November, when a paper on an ophthalmic subject was read, Mr. Fergusson asked to what extent chloroform was employed in operations on the eye, meaning the eyeball, and with what success. Mr. Dixon said that a patient might not vomit soon after the inhalation of chloroform, but he might remain twelve or twenty-four hours in a sickly, squeamish state, with but little appetite for food. This was more injurious than vomiting, for it was of the greatest importance, especially in old persons, that the nutrition of the patient should be well maintained, and the eye kept perfectly at rest.

"I have several times calmed the fears of elderly persons on whom I was about to perform extraction, and dissuaded them from inhaling chloroform, and in every instance I have been thanked for my advice. I have been asked repeatedly, even by patients, if the anæsthetic sleep does not give confidence to the operator, and enable the operation to be the better done. I can only say, that

if, in any given instance, this is likely to be true, the timid surgeon had better avail himself of every admissible assistance that is likely to restore sight to his patient. He had better narcotize him, and, if needs be, stimulate himself.

"I shall not enter into the details of preparing a patient for taking chloroform, the mode of giving it, nor the after treatment. This I have dwelt on elsewhere; but I will tell you—be certain of insuring complete insensibility before you begin to operate, that all the steps of the operation may be over before the patient is sensible, for you can have no greater misfortune than for him to awake before the termination, and to commence struggling. There is a particular point in the after treatment that must be attended to, the neglect of which has cost several eyes; and this has induced me to say in my work on the Eye: 'Except the operation for the extraction of cataract, or the division of the cornea to a like or nearly equal extent, for any other purpose, there is no operation on the eye in which well-founded objections exist to the previous use of chloroform or ether as anæsthetic agents. In the exceptions mentioned, the objection arises from fear of vomiting, and the loss of the vitreous humour. It is true, that vomiting is but an occasional effect of the use of these agents, and it can nearly always be prevented by emptiness of the stomach previous to inhalation, and which is secured by enjoining a strict fast for four or five previous hours; but, after every precaution, vomiting may occur, or severe retching, which is equally injurious.'

"I wrote this from instances that had come under my notice, and I dreaded to employ the chloroform. Subsequent experience convinced me that all necessary precautions had not been used. If the eyelids are carefully retained together with court-plaster, there is little if any ground for misapprehension, except there be that degree of vomiting which could perhaps arise only under an absence of those preliminary precautions to which I have alluded. In the case above quoted from 'The London Practice of Medicine and Surgery,' delirium tremens came on, and, in spite of the violent agitation of the man, during which the head was tossed much about, the success of the operation was not interfered with, because the eyelids were plastered together, and the eyeball protected by a guard made of wire and cloth."—*Med. Times and Gaz.*, Jan. 27th, 1855.

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47. *Opacities of the Vitreous Humour.*—Opacities of the vitreous humour, of every variety of form, belong to the commonest ophthalmoscopic discoveries in amblyopic affections. Whence do they arise? Dr. Graefe believes their causes are manifold; but he draws particular attention to one, namely, intra-ocular hemorrhages. A sudden cloud, sometimes of reddish hue, may obscure vision wholly or in part, or spots of every variety of form may float before the sight. In some cases there is complete absorption of the extravasated mass. In most instances, however, filamentous, or flocky dark spots, remain. The commonest forms are punctated, filamentous, membranous, flocky, or irregular. The first are not easily overlooked. The causes of intra-ocular hemorrhage lies, doubtless, in disease of the coats of the bloodvessels; for the contents of the globe are constantly under a state of pressure, and, therefore, the commonly exciting causes of hemorrhage are removed. The chief source is sclerotico-choroiditis posterior, which leads to chronic inflammation and distention of the capillaries in the back part of the globe. But intra-ocular hemorrhage may ensue from simple hyperæmia of the choroid, dependent upon some unknown change in the strength of the walls of the vessels.—*Med. Times and Gaz.*, Feb. 17, 1855, from *Archiv. für Ophthalmologie.*

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48. *Effusion of Blood into the Orbit.*—A labouring man, aged 19, consulted Dr. Graefe for double vision of four days' duration. A fixed expression of the left eye immediately struck the attention; its axis of vision was either in a state of unnatural convergence or divergence with the right eye, in various movements of the head. More accurate investigation convinced the author that there was complete paralysis of the musculus rectus inferior, musculus rectus superior, and musculus obliquus superior; partial paralysis of the musculus rectus internus, musculus rectus externus, and of the nervus opticus; so that the func-

tions of four nerves were affected—namely, the N. oculo-motorius, N. trochlearis, N. abducens, N. opticus. There were no cerebral symptoms whatever. Taking this circumstance into consideration, as well as the fact of the suddenness of the attack, Dr. Graefe diagnosed effusion of blood behind the bulb of the eye, with pressure on the nerves.

He also relates two cases of suddenly excited exophthalmus, from caries of the orbit; and the *post-mortem* observations on a case of paralysis of the oculo-motor nerve. The corresponding optic nerve was shrunken up to the chiasma; the oculo-motor nerve one-half thinner than the opposite; but there were interspersed opaque white blots in its tissue, which were seen to be caused by thickening of the neurilemma. In the left corpus striatum there was a softened spot. The muscles of the globe were atrophied.—*Ibid.*

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49. *New Ophthalmoscope.*—After having reviewed the different ophthalmoscopes of MM. Helmholtz, Follin and Nachet, Ruette, Jaeger, Jr., and Coccius, M. A. Anagnostakis describes that which he has invented. This instrument consists simply in a small concave mirror, round, and of a diameter of one inch and a-half, of a focal distance of four inches and a-half; the surface, tinned over, is covered by a plate of blackened copper. The centre of the mirror is pierced by a widening aperture of two lines (four millimetres) diameter. A little ivory handle serves to hold the instrument.

The patient, whose pupils have been previously dilated by a solution of the neutral sulphate of atrophine, is seated in a darkened chamber by the side of a table, on which is put a good lamp deprived of its globe. The lamp should be as close as possible to the patient, and the flame brought to a level with the eye about to be examined. The surgeon, seated in front of the patient, holds the instrument, the reflecting surface turned towards the eye to be examined, the posterior surface applied to the eye of the examiner in such a way that the eye can look through the central aperture of the mirror.

This being done, the surgeon turns the instrument a little inwards; a luminous reflection is cast upon the diseased eye. On withdrawing it a little, he sees the reflection growing smaller and smaller, until it becomes oblong, constricted, and very brilliant.

A look is then taken obliquely, through the hole in the mirror, into the eye subjected to exploration. It is in this situation, a little modified, according to circumstances, that one perceives the different alterations in the retina and in the media of the eye.

The author, who has explored in this manner a great number of diseased eyes alone or in concert with M. Desmarres, has obtained the following important results:—

1. *Exploration of the Crystalline Lens.*—Upon three patients, supposed to be suffering from amblyopia, M. Desmarres ascertained that there were three incipient cataracts, which had escaped observation. The author affirms that every opacity can be thus detected.

2. *Exploration of the Vitreous Humour.*—In three patients suffering from different degrees of troubled vision, M. Anagnostakis has observed corpuscles of a more or less deep brown colour, of different forms, and of variable volume seated in the vitreous humour. Are they inflammatory effusions, or rather traces of former hemorrhages. In a patient affected with synchisis, M. Desmarres has seen these corpuscles replaced by crystals of cholesterin.

3. *The Retina.*—The instrument has revealed injections of the vessels of the retina and sub-retinoid ecchymoses. In one patient there was a true sanguineous effusion under the whole retina, apparently from former hemorrhages. Three times did M. Anagnostakis diagnose sub-retinoid dropsies.

Finally, M. Anagnostakis thinks that in one patient he saw thickening of the retina.—*Lond. Med. Times and Gaz.*, from *Gazette Médicale de Paris*, Jan. 6, 1855.

MIDWIFERY.

50. *Secondary or Blighted Fœtus at the third month, with Fatty Degeneration of the Membranes, retained and expelled with its Living Co-twin at the eighth month of Utero-gestation.*—The following example of this was read before the Glasgow Medical Society, by JAMES G. WILSON, M. D.:—

Mrs. T., aged 32, a strong, healthy, and masculine looking woman, was, on the 21st November last, after a short and easy labour, delivered of a living female child. It presented with the breech in the right anterior dorsal position, and weighed 5¼ lbs., which, from appearance as well as from the mother's statement, indicated its age to be about the eighth month. The placenta was expelled entire in about the usual time. Patient states that five years ago she gave birth to twins, both boys, alive and at the full time, and that, with the exception of the present, she has had no children or abortion since. Her catamenia were regular during the interval, and she menstruated for the last time about the beginning of March. When three months or thereby advanced in her present pregnancy she had twice in succession a considerable sanguineous discharge per vaginam, accompanied with slight uterine pains. She, however, paid little attention to these symptoms at the time, as they did not occasion any anxiety or alarm.

The placenta, on examination, seemed generally healthy, with the exception of a few small indurated points of a tubercular character scattered over various parts of its extent. Towards the margin of the placenta there was found a small fœtus closely enveloped in a yellowish fatty-like mass, which lay in a groove or sulcus formed in the placenta by the overlapping or bulging over of a portion of its fœtal surface. The whole was connected together by a prolongation of the placental substance into the inner border of the investing membranes of this small fœtus. The bond of attachment was by a spot not larger than a shilling, which was very slender, and separated in two places. The fatty-like mass, when opened up, was of an irregular circular form, measuring eight inches in circumference, and was of the thickness of ordinary plate glass. It appears to be a homogeneous mass on cutting into it, and of a uniform density and consistence throughout. On subjecting a portion of it to microscopic examination it is found to consist of a fibrinous or lymphy substance with numerous oil globules intermixed. It is soluble to some extent in ether. There was no appearance of liquor amnii. The fœtus appears to be about the third month—is very much flattened out and disfigured, having a white exsanguine look, with no traces of incipient putrefaction. It measures 4½ inches in length. The right arm is to some extent malformed and displaced; the humerus, with its atrophied muscles, is completely incorporated with the side of the head, while the forearm is altogether above the head, and appears as if originating from it. In consequence of the pressure to which the fœtus has been subjected a large portion of the bowels protrude. The sex of the child cannot be ascertained. Proceeding from the fœtus is a small thready or filiform umbilical cord, 2¼ inches long, with the spiral convolutions formed by its vessels in great part obliterated. It is inserted into a spongy cellular substance (about the size of half a crown), much resembling placental tissue, situated at the inner border of this mass of fat, and which seemed to be the point of union with the placenta of the living child above referred to. There is, however, no distinct line of division observed between these two textures, as the one passes gradually and imperceptibly into the other. On the supposition that this substance, to which the cord was attached, was the placenta of the fœtus, the formation of the peculiar groove in which the whole was embedded may be thus explained: The two placentæ appear to have been originally united by their edges, the lesser one having ceased to enlarge, the other one gradually extended in circumference, till ultimately a flap-like portion, developed from its fœtal surface, covered in the other along with the fœtus. The relative position, however, of the double set of membranes, in consequence of some accidental laceration either during or after the expulsion of the placenta, could not be accurately ascertained. It is impossible to tell, on inspecting the prepara-

tion, what uterine attachment the blighted fœtus, through the medium of its placenta, originally possessed.

There can be no doubt, I think, but this was primarily a case of twin conception, while one was blighted at or near the third month, and the other carried to the eighth, when both were nearly simultaneously expelled from the uterus. One peculiarity in this case is the retention of a dead fœtus *in utero* for such a length of time, and its ejection a few minutes after the birth of the living child; yet when the placentæ are united in a twin gestation, it is difficult to conceive how one fœtus could be discharged without the expulsion of both. Were the symptoms of threatened abortion, already noticed, an attempt of nature to throw off and get rid of the dead child? or had they anything to do with its death, or were they the cause or the result of the partial separation that was afterwards found to exist between the two placentæ? Whatever view may be taken of these points, one thing seems evident, viz: that the symptoms of miscarriage closely correspond with that supposed period of utero-gestation at which the blighted fœtus had arrived.—*Monthly Journal of Medicine*, January, 1855.

51. *Instrument for Dilating the Os Uteri.*—Dr. Braun has contrived an instrument, which he names *colpeurynter*, for the dilatation of the os uteri. It consists of a vulcanized India-rubber bladder, from two to four inches in diameter, and four inches in length, with an India-rubber tube enclosed in horn, fitted with a brass stopcock, and a ring through which to pass a silk belt. When used, the India-rubber bladder is introduced empty into the vagina, then gradually distended by injecting cold or warm water. It is retained *in situ* by the belt fastened round one or both thighs or hip. The horn cylinder is curved in the direction of the pelvic axis; it allows only the upper end of the vagina to be stretched by the bladder: and obviates any unnecessary pressure upon the urethra or external parts. The operation is called colpeurysis. The advantages of this proceeding over other methods of dilating the os uteri, whatever the indication for that operation, are highly extolled.—*Brit. and For. Med.-Chirurg. Rev.*, Jan., 1855, from *Klinik der Geburts.*

52. *Influence of Uræmic Intoxication in the Production of Puerperal Convulsions.*—New light has been thrown upon the influence of uræmic intoxication in the production of eclampsia by Dr. Carl Braun, though some of his propositions require further testing before they can be accepted. The following is a recapitulation of the views he entertains on the subject:—

1. Convulsions in females during the generative period, depend either upon hysteria, epilepsy, diseases of the brain, mineral or vegetable poisoning, or upon uræmic intoxication.

2. The most frequent cause of eclampsia is uræmia and Bright's disease.

3. Chronic hysteria and epilepsy exert no injurious influence upon pregnancy and labour, or upon the life of the fœtus; they are never connected with Bright's disease, nor are fibrinous cylinders or a large quantity of albumen found in the urine.

4. Primary diseases of the brain, as apoplexy, meningitis, &c., are rarely causes of convulsions; and when they occur simultaneously with Bright's disease, they are the consequences, not the cause, of the convulsions.

5. All forms of convulsion, with their different causes, may occur without pregnancy.

6. They may also occur in males from any of the above-mentioned causes, except hysteria.

7. Amongst the more important causes of Bright's disease in the pregnant, may be considered venous congestion of the kidneys from the pressure of the enlarged uterus and distended abdomen, as well as the sanguineous diathesis often connected with pregnancy.

8. In *nephritis diffusa*, urea becomes detained in the blood, is converted into carbonate of ammonia, and then excites convulsions.

9. In cases of Bright's disease during pregnancy, if carbonate of ammonia be found in the blood, a speedy outbreak of convulsions may be expected.

10. No convulsions, however, occur in Bright's disease in the pregnant, provided but a small quantity of urea accumulates in the blood, or does not become converted into carbonate of ammonia.

11. The strong action of the uterus during labour is not the cause of this conversion, or of the uræmic convulsions; for these occur nearly as frequently without labour, during pregnancy and in the puerperal state; and sometimes in those who are not pregnant.

12. The abortions which frequently occur in uræmic convulsions are the effect of the uræmia, not the cause of the convulsions.

13. Eclampsia—*i. e.*, uræmic convulsions—has no immediate connection with the pains or process of labour.

14. The albuminuria cannot be the result of congestion occasioned by the convulsions, for it precedes the uræmic convulsions for days or weeks, and does not, as a rule, occur under other forms of convulsion, as from hysteria, epilepsy, &c.

15. Attacks of eclampsia *during pregnancy* may cease, and the albuminuria continue.

16. But if the attacks cease *after labour*, and there be present only a slight degree of Bright's disease, the albuminuria disappears after a few days; but continues for a longer time if the disease of the kidney be in a more advanced stage.

17. The diminution in the size of the uterus from the completion of labour contributes very much to the disappearance of albuminuria, when there is neither fatty degeneration nor atrophy of the kidneys.

18. Bright's disease (without convulsions) never gets well during pregnancy; after labour it mostly disappears within a much shorter time.

19. Albuminuria will be found in all cases of eclampsia not dependent upon hysteria, epilepsy, primary cerebral diseases, or poisoning.

20. Epilepsy may take place simultaneously with Bright's disease and uræmic convulsions; and Bright's urine may be found in an individual affected with habitual epilepsy.

21. With a frequent repetition of uræmic convulsions the fœtus dies, through the injurious influence of blood impregnated with carbonate of ammonia. The life of the fœtus is not compromised by the mechanical effects of convulsions which depend upon hysteria, epilepsy, or diseases of the brain.

22. Examination after death from uræmic convulsions always discovers Bright's disease of the kidneys; more frequently in the primary than in the latter stages (of fatty degeneration and atrophy).

23. Œdema and anæmia of the brain are as frequently found after death from eclampsia, as hyperæmia and consecutive apoplexy.

24. Reflex sensibility is entirely abolished during each uræmic attack, but during the intervals loss of consciousness generally increases.

25. The injurious effects of venesection generally in eclampsia have been observed by Kiwisch, Litzmann, Sedgwick, Blot, and King, and the uncertainty of the practice has been confirmed by our own experience. In cerebral eclampsia, however, venesection is a proper remedy.

26. Chloroform inhalations are the best means of mitigating and bringing to an end uræmic convulsions, either during pregnancy, labour, or in the puerperal period.

27. The diuretics most to be relied upon for the relief of uræmia and Bright's disease are the benzoic, citric, and tartaric acids.

28. Bright's disease during pregnancy admits of relief only, not of cure.

29. Lessening the size of the uterus, and removing the child, are the most effectual means of curing the affection of the kidneys and uræmic intoxication.

30. Exciting and hastening labour by artificial means diminishes the danger both to mother and child arising from uræmic convulsions.

31. Artificial premature delivery is not, as a rule, to be resorted to in Bright's disease; but is to be recommended in actual uræmic convulsions.

32. The most appropriate method of producing premature labour, and hastening delivery in cases of uræmic eclampsia, is by forcibly dilating the vagina by means of a caoutchouc apparatus (*Colpeuryse*).—*Brit. and For. Med.-Chirurg. Rev.*, from *Klinik der Geburtshilfe, &c.*

53. *Puerperal Mania.*—Dr. Winn, in a paper on this subject, read before the Medical Society of London (December 2, 1854), observed that the prevalence of opinions with regard to the moral management of patients suffering from puerperal mania, which he held to be not only erroneous, but dangerous, had induced him to bring the subject before the Society. He expressed his intention of passing briefly over the division of the subject which related to the physical treatment of the disease, in order that he might dwell at more length on its moral management—a question of far greater importance, and on which he considered the successful termination of a case more especially to depend. Dr. Winn remarked, that insanity might attack a patient at three periods during the puerperal state: 1, during gestation; 2, subsequent to delivery; and, 3, during lactation, as a result of protracted suckling. The observations which he was about to offer had especial reference to the second form of the malady, to that maniacal excitement which supervenes during the first months after delivery, and appeared to be the result of extreme irritability of the brain associated with great nervous exhaustion. Before entering upon the immediate subject of his paper, Dr. Winn took objection to the term "puerperal mania," used, as it commonly is, in a specific sense, and the use of which term had led to serious errors in practice. Adopted as an expression of a mere variety of insanity, it was sufficiently distinctive and appropriate; but a serious mistake was involved in the supposition, that puerperal mania is a special form of insanity, requiring a treatment entirely different from that which is laid down for the cure of mania in general. No doubt the peculiar condition of blood and the excitable state of the nervous system which obtain after delivery tended to modify a maniacal attack, and rendered it necessary that the treatment should be adapted to these qualifying circumstances. Nevertheless, the affection was essentially mania, and was to be treated as such. He then took a review of the symptoms of puerperal insanity, to show that they were perfectly identical with those of an ordinary attack of mania. The importance of this generalization would be clearly shown when he came to consider the expediency of removing puerperal lunatics to an asylum. Dr. Winn proceeded next to determine, as far as the present state of mental pathology would permit, the condition of the system which gave rise to the puerperal mania. Careful observation had led him to infer that the disease was the result of cerebral irritation, combined with nervous exhaustion, a deteriorated condition of the blood, and an imperfect nutrition of the nervous tissue. Granting these facts, an antiphlogistic mode of treatment was decidedly objectionable. The classes of remedies most likely to prove beneficial were, sedatives, depuratives, and tonics. A few leeches might occasionally be employed to allay the erethism of the brain, but on no account as a depleting measure. Venesection and blistering were positively injurious. A nourishing though not a stimulating diet was generally required; while opiates, aperients, and diuretics, would all be found more or less useful. It was, however, on the moral treatment that the recovery chiefly, if not wholly depended. This should be precisely the same as that which is generally indicated in any variety of mania. An efficient nurse must be provided, capable of maintaining a kind but firm control over the patient. Every precaution must be adopted to prevent self-injury. The greatest tranquillity must be preserved, and the patient's friends and relatives rigorously removed from her presence. The great object was, to break the morbid current of thought by a seclusion more or less complete, and thus give rise to emotions and ideas entirely new. The most important point for consideration, however, was the question of the patient's removal to an asylum when the ordinary remedies had failed to afford relief. However inexpedient and culpable it might be to hurry a patient to an asylum at the outset of puerperal or any other form of mania, still a period might arrive when delay in having recourse to this measure would subject the patient's health and life to the greatest danger. Dr. Winn stated, that the general opinion was opposed to the removal of a puerperal lunatic to an asylum. To combat a dogma, fraught with such imminent peril, was the principal object of his communication. Dr. Forbes Winslow had clearly established the fact, in his valuable treatise on the Incubation of Insanity, that it was only during the earlier periods of an attack of mania that a cure could be expected from the use of appropriate remedies.

By a strange perversion of a general principle, this important truth had been commonly lost sight of in the treatment of puerperal mania. In support of this assertion, Dr. Winn quoted passages from the works of various authors who had condemned the removal of puerperal patients to an asylum. One of these authorities had made use of the very objectionable expression "madhouse." Dr. Winn commented very strongly on the use of such a term. It was appropriate enough for the asylum of the darker ages, when every description of cruel restraint was practised; but singularly inapplicable now, when humanity and science were alike enlisted to allay the sufferings and meliorate the condition of their inmates. It was to be hoped that, ere long, even the milder term of "lunatic asylum" would be abolished, and the far more appropriate title of "Hospital for the Cure of the Insane," as recommended by Dr. Winslow, be generally adopted. He next related cases from his practice to prove the truth of his views. One patient whom he attended with Mr. Meeres, proved in the most striking manner the advantage of an early removal. Although the patient had been ill only a week, Dr. Winn thought it advisable to send her to an asylum at once. Her recovery was rapid, and she has been confined again since her return to her home, but has fortunately had no recurrence of the disease. Another case which he mentioned showed, on the other hand, the bad effects of delay. This patient had been ill for many months before Dr. Winn saw her, and all the routine practice had been tried in vain. Had early removal been adopted, he thought the case would have speedily recovered. The returns of lunatic asylums showed, that the treatment of puerperal mania in these institutions was pre-eminently successful. Dr. Webster's tables proved, that more recoveries from puerperal insanity were reported than from any other form of lunacy. Dr. Boisragon, of the Cornwall Asylum, had kindly furnished Dr. Winn with the result of his experience. He (Dr. Boisragon) stated, that the majority of cases recover; he had not, in fact, discharged a single case that was not convalescent. Some, who admitted the efficacy of this plan of treatment, disapproved of it on the ground, that the recollection of having been confined in an asylum would produce an injurious effect on the mind of the patient in a subsequent labour. Such an instance Dr. Winn had never witnessed. Another common prejudice, which interfered with the timely removal of a patient to an asylum, was the dread entertained by the friends of the individual lest some sort of opprobrium would be incurred by her having been the inmate of what they call a "madhouse." This prejudice, the offspring of ignorance and mistaken delicacy, was fast passing away. The supposition that puerperal mania was generally a very transient affection was another erroneous opinion, which had occasionally interfered with the early removal of patients. In conclusion, Dr. Winn wished it to be clearly understood that he did not recommend the hasty and indiscriminate removal of patients; at the present time he had a patient, affected with insanity dependent on uterine derangement, under his care, and who had been ill for several months before he saw her. In this case, physical treatment had almost completed a cure, and removal to an asylum had not been had recourse to. In the humble walks of life, cases of insanity should be generally removed at an early period to an asylum. For rich patients this measure was not always necessary; they could obtain a quiet residence, all the comforts, and many of the advantages of an asylum. Not so with the poor; confined, perhaps, to a close room in a narrow and noisy street, insufficiently nourished, and badly nursed, the poor patient was cut off from all hope of a cure. For cases of this description an asylum offered the only chance of a cure.

Dr. Webster concurred with the author in most of his views and recommendations; but he considered that much benefit was derived by patients, especially those in the lower ranks of life, becoming inmates of lunatic asylums. 167 cases of puerperal mania had been received into the Bethlehem Hospital during the last ten years. Of those cases in which the disease commenced immediately after delivery, a very large porportion (65 per cent.) had been cured. In the cases arising from protracted lactation, the cures were not so numerous. The mortality attendant upon puerperal mania had, of late years, considerably diminished, it being now only 4 per cent.; and this, he believed, was owing to the discontinuance of the practice of bleeding, and better treatment generally.

In the higher ranks of society, where puerperal mania was more common than in the lower, it was not so necessary that patients should be sent to an asylum; but even they might sometimes be benefited by such a removal from familar scenes. The disease resembled delirium tremens, and should be treated much on the same principles.

Several members expressed objections to sending cases of puerperal mania to lunatic asylums.—*Med. Times and Gaz.*, Dec. 9, 1854.

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54. *Discussion at the Imperial Academy of Medicine on M. Depaul's Report on the Treatment of Displacements of the Uterus by Intra-Uterine Pessaries.*—After some remarks from M. Piorry, the discussion on this report, of which an abstract was given in our preceding No., p. 257, *et seq.*, was opened by M. Malgaigne, who, while admitting the zeal and talent of the reporter, regretted to find so much party spirit in the report, which more resembled the impassioned argument of a special pleader, than a plain and impartial judgment of facts. He could not consider as *repugnant to common sense* a treatment, which had been practised by Velpeau, Amussat, Simpson, Huguier, and Valleix. It appeared even from the report that in some cases, the intra-uterine pessary had been useful, and sometimes it had procured instantaneous relief, showing that the symptoms were really owing to displacement, and he had often witnessed cases of descent of the uterus, where the use of a pessary had put a stop to all the symptoms; the hypogastric belt also, by replacing the uterus, had often produced the same beneficial results. It appeared to him, that the important inquiry was to establish a differential diagnosis of uterine affections; since the same symptoms were ascribed by some exclusively to displacements, by others exclusively to enlargement (*engorgement*), by others to granulations, ulcerations, etc. etc. In neuralgia of the uterus, the pessary, he thought, might certainly be serviceable. In short, the intra-uterine pessaries ought not to be entirely rejected, but their use should be restricted to cases without inflammation, acute or chronic, the instrument should be employed cautiously, and not be allowed to remain permanently; and, above all, the treatment should be controlled according to rational indications; the practitioner should know *what he does and why he does it.*

M. Huguier, who had not signed the report in consequence of differing from M. Depaul in some of his conclusions, made a full and able statement of his views. With regard to the intra-uterine pessaries, he thought that they ought not to be entirely rejected; but that their use should be confined to those exceptional cases of displacement giving rise to serious symptoms, which resist other means of treatment; that their application should be made with great caution and carefully watched. He especially commends their efficacy as emmenagogues, when other means have failed. On the pathological question, he explained the circumstances under which uterine displacements occasion no symptoms; and after distinguishing the local and sympathetic affections which are occasioned by inflammations, etc. of the uterus, he defines those symptoms which properly belong to and are occasioned by uterine displacement, particularly disorder of the menstruation, and distressing affections of the bladder and rectum (frequent micturition, retention, obstruction of the rectum, etc.). He considers that deviations, especially ante and retro-flexion, may occasion sterility; and maintains that the displacements are undoubtedly curable. In conclusion, M. Huguier relates his experience of the general and local treatment of the displacements by pessaries, etc., and describes the methods by which he has succeeded in replacing the uterus when retro-flexed by the insertion of pledgets (*mêche*) of lint in the rectum.

M. Hervez de Chegoin was of opinion that the displacements may occasion most distressing symptoms, which are only to be relieved by replacing the organ. But this result ought to be accomplished by vaginal pessaries external to the uterine cavity. The intra-uterine pessaries had produced so many serious accidents, that he was surprised any one could follow a method attended with such risks. He considered that they ought to be altogether dismissed from practice.

M. P. Dubois denied that the vaginal pessaries recommended by M. Chégoin,

could ever effectually replace a retroverted uterus; consequently, he considered the intra-uterine instruments to be perfectly rational, and indeed, the only means capable of effecting the end in view. The only question was, are they without danger? It is certain that their introduction frequently gives rise to severe, sometimes even fatal consequences; sometimes the instruments cannot be tolerated; while, in some cases, they are quite innocuous, and occasionally their use is attended with immediate, or even permanent, relief. M. Dubois, however, maintains that in no instance do they ever attain their proposed object; the uterus is never replaced permanently in position by their use. This assertion he supports both on theoretical grounds, and by the result of his examination of cases treated by himself, by Valleix, and by Simpson. Although, however, the displacement is never removed by this method, the uterine symptoms may be alleviated or even cured by it. These results are owing to the pessary relieving the weight of the uterus, and the tension of its ligaments; to the alteration in the morbid action by contact when the uterus is inflamed, and to the alteration of its morbid sensibility in cases of neuralgia. In the latter class of cases the method is of particular service, and often relieves all the symptoms, even where no deviation had existed. Many of M. Valleix's cures occurred in cases of neuralgia. Consequently, the intra-uterine pessaries ought to be retained in practice, not as directors of the position of the uterus, but as modifying agents which may be placed in competition with other efficacious means already known as such, cauterizations, douches, etc. And in all cases, the general treatment should be attended to as well as the local, a principal condition to which M. Valleix owes much of his success.

M. Cazeaux expressed his opinions at great length. He objected to the report for stating the unfortunate and fatal cases, and omitting the successful ones. He admitted that by removing the displacements, both the symptoms which it occasions, and even the concomitant lesions, granulations, etc. may be cured. In opposition to M. Dubois, he maintained the efficacy of the (M. Chégoin's) vaginal pessaries in retroversion; and he considered inflexion to be a congenital condition in the child, adopting the opinion of M. Boulard, Follin, and Verneuil. He is opposed to the intra-uterine pessary, as mostly useless and often dangerous, but he considered that the uterine sound, with proper precautions, may be rendered safe, and is of real service.

M. Gibert maintained that too much importance had been attached to uterine diseases altogether, and great illusions existed in regard to them; that they often depended on constitutional states; and he mentioned instances of several diseases of the uterus, where erroneous and overstrained views of their pathological consequence had led to methods of treatment as severe and dangerous as they were useless and uncalled for.

M. Velpeau, in one of the most important addresses delivered in the discussion, entered at considerable length into the details of the question, and opposed, in decided terms, the conclusions of M. Depaul's report. He passed a high eulogium on M. Valleix, and attached great importance to the results of his practice. He maintained that practitioners have not exaggerated the effects of uterine displacements, nor have they attributed to them those affections which resulted from other pathological conditions. Wherever disease of the uterus, enlargement or engorgement (if not mistaken for anteflexion, as has often happened), or ulceration, or leucorrhœa occurred, no practitioners could overlook these conditions or confound them with the effects of simple uterine displacement. And, although there may be cases of uterine displacement, without urgent symptoms, just as there is hardly any important organic disease the symptoms of which may not be latent, still no one could doubt the serious affections which may be caused solely by uterine displacements, and may be removed by replacing the uterus. M. Velpeau was convinced that, although these deviations yielded, in general, a favourable prognosis, yet that they produced, especially in nervous women, affections of real danger. M. Velpeau next examined the means of remedying these affections, enumerated by M. Depaul and others; and, while recognizing their usefulness in certain cases, he was quite persuaded of their radical inefficiency. In consequence of this conviction, he had himself long ago attempted the use of intra-uterine pessaries,

but had desisted after the trial of a few months, being deterred more by theoretical considerations than by the results of practice. He now thought that the dangers of these pessaries had been exaggerated, and might be avoided by more careful and experienced manipulation. The intra-uterine pessaries had not indeed solved the difficulties in the radical treatment of uterine displacements, but they had already rendered great services, and would with further experience render more; they ought not to be rejected, but their employment ought to be regulated judiciously; and he trusted to the experience of M. Valleix to disclose the advantages and the disadvantages which belonged to this method of treatment. M. Velpeau was of opinion that the conclusions of the report ought to be suppressed, and that the Academy could only vote in reference to the cases brought forward by MM. Broca and Cruveilhier.

M. Depaul explained that the conclusions already given were merely the summing up of his report, and that he would, after the discussion was finished, submit to the Academy a series of conclusions embodying the unanimous opinion of the committee.

M. Amussat said that the idea of introducing bougies (*tiges*) within the cavity of the uterus, first suggested itself to him in 1826, on seeing M. Recamier use the uterine sound for diagnostic purposes. Among the first cases in which he used the pessary was that of a woman with anteversion; he introduced a pessary with an ivory stem into the uterus, and allowed the patient to go about with the instrument *in situ*. Inflammation supervened, and the patient died. Deeply grieved in consequence of this mishap, he tried to remedy all displacements by extra-uterine instruments. He employed for this purpose conical pessaries, which resembled ivory cupping-glasses; but these were found productive of so much pain, that he had to abandon their use. For thirty years he had used both intra and extra-uterine instruments. (Here M. Amussat exhibited to the members of the Academy his various instruments—some made of ivory and steel, and others of india-rubber, elastic gum, etc.) He was discouraged, however, by the general results of instrumental treatment. He had heard of many spontaneous cures of uterine displacements, resulting from local inflammations and ulcerations having produced adhesions between the uterus and vagina, and this led him to imitate nature, and to attempt their cure by gently cauterizing the posterior part of the cervix, that by the adhesions formed during the cicatrization of the sore, the displacements might be radically and permanently cured. The successful results of this mode of treatment had far surpassed his anticipations. Relaxation of the vagina and procidentia of the uterus often rendered the application of the caustic somewhat difficult; but by putting the fingers into the rectum, and simultaneously using the sound, the position of the uterus could be sufficiently maintained during its application, and by a pessary thereafter. He related several successful cases illustrative of this mode of treatment in cases of retroversion. He had not found cauterization of the anterior lip of the cervix to be beneficial in anteversion. In such cases he used a cup-shaped pessary (*en gobelet*), to support the uterus. In conclusion, he believed the indiscriminate use of pessaries for the cure of displacements to be prejudicial; he considered the employment of intra-uterine instruments as dangerous in some instances, and as productive of merely temporary benefit in the great majority of cases; but he characterized his own method of treatment as free from all danger, and as permanent in its results—except after pregnancy.

M. Malgaigne again spoke at great length. He agreed with M. Hervez de Chégoin, in considering that uterine displacements occasioned great pain, and also engorgement, and that these disappeared after the replacement of the organ. But he affirmed that it was impossible to recognize engorgements of the uterus apart from, and independent of, displacements. He commented, with some severity, on the statements made by Gibert, in which that gentleman had underrated the importance of uterine affections, and attributed their symptoms to general pathological conditions of the system. Referring to what had been said by M. Dubois, he alleged that neuralgia of the uterus did not appear to be a very common complaint, for although he *had* seen cases of it, they were very few in number; but he bowed deferentially to the experience of Dubois. He

maintained that the pain occasioned by pure displacements of the uterus was removable by mechanical means; and that the recumbent posture caused its cessation. This circumstance he regarded as pathognomonic of the affection. He looked upon the speculum as of little use in the diagnosis of displacements, as he had known such cases when examined by means of it, diagnosed as ulcerations of the cervix, and treated needlessly with cauterants. M. Huguier had alleged that displacements, when trifling in amount, occasioned no pain, but herein he was in error. The suffering occasioned by displacements was by no means regulated by their extent, but solely by the special susceptibility of the patients in whom they occurred. Great prolapsus might occasion no pain, while trifling deviations of position might be quite intolerable. M. Cazeaux had said: "Congenital inflexions are seldom the cause of pain, while those occurring accidentally are always the source of suffering, which is sometimes very acute." He asked M. Cazeaux for some proof of the existence of congenital inflexions, and by what means he distinguished them from the others. He could conceive of no way except saying to the patient: "Madam, have you had this inflexion since your birth?" After passing a high compliment on M. Velpeau, he made some remarks on certain portions of his address. Velpeau had stated his belief that the displacements occasioned pain in the round and broad ligaments, which he said were filled with nervous filaments derived from the great sympathetic, and continuous with the mesenteric nerves. This doctrine Malgaigne condemned as unphysiological. He did not agree with Velpeau in thinking that "no experienced practitioners could overlook these conditions." The present discussion was the best proof of the possibility of such mistakes. As to the hypogastric belt, which had been originated and greatly used by Velpeau, he affirmed that it could always be borne by patients who had simply uncomplicated displacements; and that where it was not borne well at first, it seldom was so even after usage for some time. *Can uterine displacements be replaced?* Undoubtedly, for he had seen three cases of retroflexion cured spontaneously. There was no difficulty in the replacement of so mobile an organ; and in ordinary cases, where there were no adhesions, the instrument when introduced into it, replaced it instantaneously, and without any effort. He could not see the advantage to be gained by allowing the instrument to remain (as proposed by Simpson, Valleix, and others) within the uterus, for days, weeks, or months. What was the purpose of this? To give tonicity to the ligaments? They were not placed in their normal direction. If it was in order to allow the intestines (which were normally interposed between the uterus and the rectum, and keep it *in situ*) time to slip behind the organ again, it was unnecessary, as this was the work of a moment. He was of opinion that the instrument for replacing the uterus should not be allowed to remain within it more than a few minutes at the utmost. *When the uterus is replaced, can we maintain it permanently so?* That was much less probable, for the organ having once been displaced, was very prone to become so again. He believed that some complete recoveries had been effected, but that in *no* case was a cure certain. Seeing, then, that a cure was so very uncertain, and palliation alone could be hoped for, he considered it was neither prudent nor reasonable that we should subject our patients to the risk of danger and death. He concluded thus: "I have only one word more to add, and it is this: If, unfortunately, I had to treat my wife or my daughter for a uterine displacement, I should not choose the more dangerous of two palliative measures, and most assuredly, I should not subject them to the use of the intra-uterine instrument."

M. Ricord desired to make a few remarks on the treatment of certain hyperæsthetic conditions of the cervix and vagina, which were sometimes consequences or complications of various displacements of the uterus, with or without some other morbid condition. There were many cases of displacement where the pain was alleviated or removed by lying in a certain position. He considered that much of M. Valleix's success was due to his displacing and isolating the uterus from its painful position, without making pressure (*sans prendre un point d'appui*) on the sensitive hyperæsthetic parts. For a serious case of this nature, in which all the resources of therapeutics had failed in the ablest hands, and in which *considerable benefit* had been derived from the use of M. Valleix's

instrument—he had invented a new instrument, whereby the uterus was isolated, supported, and maintained in a suitable position, without the instrument entering the cavity of the womb, or touching the vagina. Its use was quite painless and free from bad consequences in cases where the cervix could be touched by an instrument or by the finger of the accoucheur. This instrument consisted of a forceps, which was introduced by the aid of a speculum, and the points of which seized the cervix. By means of a ring, moved by a screw, the uterus could be raised to any desired height; and the whole apparatus was fixed by means of the plate (*plastron*) of M. Valleix's instrument. In conclusion, he considered, with MM. Paul Dubois and Velpeau, as well as with M. Huguier and with M. Malgaigne in his first speech, that the mode of treatment practised by Simpson and Valleix was productive of great benefit, when applied to cases requiring it, and that consequently it should not be rejected from practice.

M. Robert said he rose with some hesitation to address the Academy, but having been a member of the commission for whom M. Depaul acted as reporter, he considered it right to state wherein he differed in opinion from M. Depaul. He did not, with M. Depaul, consider that the influence exercised by uterine displacement on the health of females, had been at all exaggerated. In his opinion, there existed two classes of deviations—the one comprising such as were primitive, and resulted from accidental and occasional causes; the other, such as were secondary, occasioned by different lesions of the uterus, more especially of the body of the organ. The former class of cases were observed to occur thus: "A woman in good health makes a sudden or violent effort, or falls on the breech; she feels at the time a crack and a painful sensation in the lower part of the abdomen. From this time she suffers more or less pain; menstruation becomes deranged, and is either too scanty or too profuse. She consults us, and we find retroversion; the uterus is painful, tumefied, etc." He observed that such cases should not be neglected, as they did not always remain uncomplicated, and functional disorders, as well as organic lesions, were liable to result from them. The latter class of cases followed chronic metritis, uterine catarrh, and inflammation of the cellular tissue surrounding the uterus. They occurred after protracted labours, and especially after miscarriages; and were observed in females of lymphatic temperament, in whom uterine congestion occurred without signs of phlegmasia. The fundus of the uterus was generally at fault in these cases; and was often so voluminous and heavy that it swayed backwards and forwards. In retroversion, suffering resulted from the interrupted circulation in the uterus, from the obstruction offered to the passage of feces, and from the pressure of the organ on the sacral nerves. He expressed astonishment at what had been said by Depaul and Velpeau as to the ease with which displacements could be diagnosed. He had studied diseases of the uterus carefully during the twenty years which he had been surgeon to the Hôpital de Lourcine, and he still often found great difficulty in diagnosing displacements. He considered the pathognomonic sign of displacements adduced by Velpeau and Malgaigne; viz: freedom from pain in the recumbent posture and return of uneasiness on rising—applicable only to cases of anteversion complicated with procidentia (*abaissement*), and not to retroversion.

He had for a long time been in the habit of introducing dilating bougies into the cavity of the uterus, and never saw any bad effects; and he had also tried to replace the organ by means of an instrument like that shown by M. Velpeau, but without success. He had also employed Simpson's pessary. He first used it in a case of retroversion which had resisted all other treatment. No evil result occurred during its use, but, on the contrary, the catamenia, which had been absent more than three months, returned in normal quantity on the fourth day after its application. It was worn for a period of six weeks, and during this time, a second menstrual epoch occurred, without any pain or inconvenience. At the end of the six weeks it was withdrawn, and he then found that the uterus had regained its normal position. The retroversion returned, however, and the pessary was again introduced. The patient left the hospital, and went about in Paris for ten or twelve days without experiencing any pain; but feeling it uneasy after that time she applied for its removal. Since then he had never seen her. He used Simpson's instrument in a second

case of retroversion, but as it induced a continual discharge of blood, he was obliged to withdraw it on the tenth day. In a third case of retroversion, occurring in a woman of sanguine and irritable constitution, he used this pessary. He had to withdraw it, however, at the end of three hours, inasmuch as it caused severe uterine pains, accompanied by vomiting. He tried it again three times, at intervals of several days, but without success. He had therefore abandoned the use of the instrument. Although injurious results had followed the use of this instrument, he admitted that it was proved by incontestable facts that in very many cases, where all other measures had failed, this mode of treatment had been productive of complete and lasting success. He thought that it would be premature to exclude this instrument from practice, but he considered it very desirable that its mode of action was better understood, and above all, that we knew accurately all its indications and counter-indications. This knowledge would render its employment more efficacious, and would diminish the number of accidents resulting from its use.

M. Depaul then replied at great length, and maintained that his report had been impartially prepared. M. Huguier had said that intra-uterine pessaries should be used in "those exceptional cases of displacement, giving rise to serious symptoms, which resist other means of treatment." If displacements occasioning severe symptoms were rare, how much more so must be those cases which resisted treatment? He could fearlessly affirm that such cases were hardly to be met with during the course of a long career. As regarded the use of pessaries for emmenagogue purposes, he could say that cases of amenorrhœa affecting the life and health of women were very rare, and that those which resisted ordinary medical treatment were still more exceptional. He believed that the sanguineous discharge provoked by the introduction of these instruments was *not* the catamenial fluid, but a true uterine hemorrhage, which had been mistaken for menstruation. As to the influence of displacements upon the health, he believed that statistics, collected from a number of rigorously observed cases, were necessary to set the question at rest. He still remained convinced that there was great exaggeration in the opinions of the partisans of mechanical replacement; and he thought that he had proved this from their own observations. He had never denied that displacements or flexions might occasion sterility, and he believed this to be due to an alteration both of structure and of form. Were uterine displacements incurable? No one had affirmed this absolutely. There were a great many displacements, especially congenital inflexions, which resisted all treatment, but there were others which were easily removed. The first question he always asked himself when called to treat such cases, was: "Is the tissue of the organ sound?" If not, he tried to remedy the disease before the displacement. He did not mean absolutely to assert that uterine displacements were of no consequence, or that they caused no accidents. He had seen enough of these diseases to know that it was not so. He agreed with M. Hervez de Chégoin that many displacements resulted from diseases of the uterus, but he did not consider, with him, that cases of uncomplicated displacement, producing distressing symptoms, were at all common; he believed them to be very rare, but he did not deny their existence. It was far more usual to find displacements—flexions and versions—existing to a considerable extent, without in the least affecting the health, or leading the women in whom they occurred to suspect their existence. As regarded the introduction of intra-uterine pessaries, he might mention that M. Michon had not only abandoned their use, but had expressed scruples about introducing foreign bodies even into the cervix, inasmuch as he had seen peri-uterine inflammation and the formation of abscesses follow the introduction of a hollow sound. He agreed with the principles of treatment laid down by M. de Chégoin, and considered that mechanical treatment should never be resorted to until all other pathological conditions, existing simultaneously, had been removed; that we should not confine our attention *solely* to the local affection, but by all suitable means endeavour to remedy the general health, and those derangements of innervation, nutrition, and circulation which were so common in such cases. He believed further that, in certain cases, it might be advantageous to associate with the local treatment of some uterine affections, the use of me-

chanical means, which by fixing (*immobilisant*) the organ without iritating it, might aid in removing chronic inflammations, of a partial or general character. He protested against the injustice of M. Cazeaux's accusation of having quoted only the unfortunate, and suppressed all the successful cases; he had drawn his data from all the cases he was cognizant of; and he knew of no really successful ones. He appealed to the statistics of M. Gosselin which he had quoted, for the truth of his assertion, that many uterine affections—even the most serious—might exist without giving rise to any uneasy symptoms. Displacement was a simple unimportant deformity. M. Cazeaux had alluded to catarrhs, granulations, ulcerations, neuralgias, etc. coexisting with displacements. In such cases these lesions should be attacked before the displacements; and in sixty cases he had treated on this principle there were only three in which any bad symptoms remained after two or three months. He advanced a series of arguments and statistics, at great length, to prove the fallacy of the assertion—made originally by MM. Boulard and Verneuil, and quoted in the speech of M. Cazeaux—that anteversion was the normal condition of the uterus in children and women who had never borne children. He had shown by his researches that intestinal expansion, in the dead body, might raise up the organ, and so deceive observers. But he admitted that there was a *predisposition to anteversion* at all periods of life. He regretted that M. Cazeaux had not stated, for the enlightenment of MM. Huguier, Valleix, Nonat, and others, the "proper precautions" which rendered uterine catheterism safe. He did not deny the usefulness of the uterine sound (*hystéromètre*); he considered that it was wrong to make the use of it general, and thought its employment should be confined to the *few* cases in which it was indispensable. With considerable asperity, Depaul next attacked the assertion which had been made by Cazeaux, that there was no such thing as inflammation of the tissue-proper of the uterus; and he quoted cases and authorities to disprove it. He alluded to the statement made by M. Dubois, that the uterus was never permanently replaced by the use of instruments, and asked if, in the event of this statement being true, it did not militate against the use of intra-uterine pessaries. After the cases cited by M. Dubois and himself, he thought there could be no doubt of the fact that these instruments were useless. (*"Les redresseurs ne redressent pas."*) He believed that a modified form of the pessary might perhaps, as Dubois said, relieve uterine neuralgia; but he did not hesitate to express his opinion, that the evil results and fatal cases caused by the intra-uterine pessary ought to exclude it from use in the treatment of all serious uterine phlegmasiæ. He next alluded to M. Malgaigne's speech; and considered as very inconclusive and unsatisfactory the test he gave for the diagnosis of displacements, viz: cessation of pain on assuming the horizontal posture. He also objected to Malgaigne's detestation of the speculum, and of vaginal examination in such cases. He had accepted an invitation from M. Malgaigne, on the 2d July, to go to the Hôpital St. Louis, to see two cases there which had been cured by M.'s hypogastric belt. They were said by Malgaigne to be cases of simple displacement—the one a case of anteversion, the other of retroversion. On examining the patients manually and by means of the speculum, he found considerable ulceration of the cervix to be present in both cases; in the first case there existed, in addition, slight anteversion and considerable prolapsus, in the other there was no displacement whatever. He agreed with all that had been said by his learned master, M. Velpeau, in praise of the high scientific character and general probity of M. Valleix; but he dissented from the opinions expressed by Velpeau as to his practice. If Valleix had had cases proving the successful use of intra-uterine instruments, he was astonished that he had not published them. Velpeau had attributed most of the evil results of pessaries to errors of practice and diagnosis, and to want of expertness and ease in manipulation. He could not see wherein lay the difficulty of introducing a bougie into the uterine cavity, and could not imagine practitioners like MM. Valleix, Nélaton, Huguier, Aran, etc. being deficient in proper precautions or expertness. What always astonished him was that the uterus should be so tolerant of instrumental interference, when it was recollected how easily it was irritated by the presence of a small foreign body, and how the presence of a simple clot

or polypus within its cavity induced violent contractions of it, which only ceased when the offending substance was expelled. There was a great difference between these foreign bodies and an intra-uterine pessary formed of solid, inflexible material. It was said that the end of the stalk should not touch the fundus uteri; but it was forgotten that this organ had, properly speaking, *no* cavity; and, however short might be the instrument introduced, it acted on the organ by all the parts of its surface, and especially by its point, against which the uterus was continually being impinged, either by its own inherent force of contractility, or by the impulsions given to the organ by muscular movements and the pressure of the intestines. M. Velpeau had concluded his speech by expressing a hope that M. Valleix would decide the question as to whether these instruments should or should not be used. M. Valleix had already done so; but his opinions were dangerous, and therefore he (M. Depaul) could not accept them. He concluded by laying before the Academy the following conclusions made in the name of the commission, and carried by a majority, with the exception of the third, in which M. Depaul said he was at variance with his colleagues:—

1. *The cases reported to the Academy by MM. Broca and Cruveilhier, along with numerous others on record, prove that the use of the intra-uterine pessary may often occasion serious accidents, and sometimes even death.*

2. *In the rare cases in which this instrument has appeared productive of advantageous results, it is not proved that it had always acted by replacing the uterus.*

3. *In some exceptional cases where uterine displacements* (déviations) *have occasioned serious functional derangements, and where all known therapeutic measures have been tried in vain, the application of the intra-uterine pessary may be tried as a last resource.*

4. *The commission has the honour to propose a vote of thanks to M. Broca, for his very interesting communication which has originated the late debates; and also to M. Cruveilhier.*

The President put the first conclusion to the vote.

M. Velpeau.—This conclusion appears to me to have been adopted; but it is too severe. Instead of saying "the use of the intra-uterine pessary may *often*," I would substitute the words, "may *sometimes* occasion," etc.

The President.—It is proposed to suppress the adverbs altogether. It should be read: "The use of the pessary *may* occasion," etc.

The President then put the first conclusion to the vote, with the word "*often*" suppressed. It was adopted in this modified form.

The *second* conclusion was carried unanimously, without any modification.

The *third* conclusion was then read, and M. Gibert demanded its suppression.

M. Velpeau.—I insist on its being allowed to remain.

M. Cazeaux.—I am convinced that the use of the intra-uterine pessary is always useless, and sometimes dangerous. I cannot adopt this conclusion.

The President.—It contains the words: "as a last resource." For this purpose we may try anything (*laughter*).

Several members proposed the suppression of the conclusion, and the President proposed to put it to the vote.

M. Jobert requested the suppression of the words "as a last resource" (*comme dernière ressource*).

M. Velpeau supported this amendment.

The conclusion thus modified was put to the vote, and was rejected.—*Monthly Journ. Med. Sciences*, Aug. and Sept. 1854.

FOREIGN CORRESPONDENCE.

Parisian Medical News. Communicated in a letter to the editor, from E. WARREN, M. D.—M. Bernard has been making a number of experiments in regard to the secretion of sugar by the liver. In a recent lecture, he took a dog which had eaten nothing for four days, and examined the blood of the vena porta and sub-hepatic veins. In the first he found no sugar at all, but in the other he discovered that substance in considerable quantity. From these facts, he drew the necessary inference that the faculty which the liver possesses of forming sugar exists when no aliment has been taken into the system. He affirmed, in this connection, that this creative power of the liver was not at all destroyed in animals who had fasted even for eighteen days.

He took another dog, the pneumogastric nerve of which had been cut twenty-four hours before, and, after making an examination of his blood, demonstrated that it contained no sugar at all. He found none of that material in the hepatic veins, and scarcely a trace of it in the liver itself. He said that if the dog were examined at the expiration of forty-eight hours not a particle of sugar could be detected in that organ; from which he concluded that by arresting the action of the nerves which preside over the functions of the liver, its power of forming sugar was entirely destroyed. He then laid down the general law that "all the secreting organs are directly under the influence of the nerves, and that their secreting powers may be increased or diminished by exciting or depressing the nervous system."

In illustration of this proposition, he proceeded to show that he could at pleasure either increase the amount of sugar secreted, lessen or arrest this peculiar function of the liver, or force that organ to produce a different substance, which might subsequently be transformed into sugar.

1. As a means of demonstrating his ability to increase the amount of sugar, he took a rabbit, and punctured its medulla oblongata, at a point situated between the origins of the acoustic and pneumogastric nerves. He then affirmed that in an hour the rabbit would be diabetic, in consequence of the irritation of the nerves thus produced. At the end of that time, he made an examination of the urine of the animal, in the presence of the whole class, and found that sugar presented itself in considerable quantity in that secretion. He accounted for the existence of sugar in the urine by the fact that the liver is the great reservoir of sugar in the economy, and that when the quantity of that substance is increased in this organ, the blood becomes saturated with it, and the urinary secretion is impregnated as a necessary consequence. He said that he had made many experiments for the purpose of ascertaining whether the kidneys themselves are capable of secreting sugar, and to settle the question of its transmission outwards by other passages. The result of his researches was a conviction that, like everything else excreted from the economy, it manifested a certain preference in its selection of organs of elimination and channels of escape. For his experiments in this direction, he had employed: 1, different kinds of sugar; 2, iodide of potassium; and, 3, prussiate of potash, as the substances most easily recognized in liquids. When these materials are taken into the system, all correct observation proves that they are found in certain secretions and not in others.

Sugar finds its way out but by two passages: 1. By the kidneys; 2. By the gastric juice. Never mind how great the quantity of sugar injected in

the veins, it does not make its exit from the body by saliva, the pancreatic juice, the tears, or the bile, but always selects the urine and gastric juice as the vehicles of its passage. M. Bernard had often examined the saliva of persons affected with diabetes at La Charité, and had never been able to find in it a trace of sugar. He accounted for the "sweet taste," in the mouths of such persons, by saying that the sugar which circulated in the blood came in contact with the gustatory nerves, and produced the sensation. As an evidence of this, he stated that Magendie had found that, when bitter substances had been injected into the veins, a disagreeable taste was immediately produced in the mouth. He also explained satisfactorily why certain observers had found sugar in the expectorations of persons affected simultaneously with diabetic and pulmonary disease. He said that the sputa of such persons differed materially from ordinary saliva; the one being the mucus produced in the bronchial tubes; the other, the secretion from the salivary glands of the mouth. The first, when analyzed, sometimes gave a slight saccharine reaction; the second was always entirely free from the slightest trace of sugar. As a means of showing that substances introduced into the economy are not eliminated indiscriminately by all the secretions, he performed the following experiment:—

He took a dog, into whose parotid gland he had previously introduced a small metallic tube, and injected his jugular vein with a liquid composed of a mixture of sugar, iodide of potassium, and prussiate of potash. After waiting a short time, he procured a quantity of saliva and urine and subjected them to certain chemical tests. The iodide of potash alone presented itself in the salivary secretion, whilst all three of the substances were found in the urine.

M. Bernard believes that the presence of sugar in the gastric juice had deceived and misled many observers. The liver, and not the stomach, is the great sugar generator. The fact that wine in the stomach of diabetic persons becomes sweet after the lapse of a certain time, does not prove to the contrary in the least degree, as has been supposed by Mr. McGregor and others. The gastric juice, which is impregnated with sugar, mixes with the wine, and imparts a saccharine taste to it. The gastric juice also allows the passage of the prussiate of potash, as can easily be established in the following manner: Let some aliment containing iron be passed into the stomach, and a preparation of prussiate of *potash* injected into the veins. Then an examination of the contents of the stomach will show that the prussiate of *iron* has been deposited among them.

These experiments, with their explanations, and the deductions from them, constituted the substance matter of one of the most interesting lectures delivered by M. Bernard since the opening of his present course. He is not a fine speaker, but he is undoubtedly the most successful experimenter, and the first practical physiologist in the world. His rooms are always crowded with students, both French and foreign, and he never fails to interest and instruct all who give him their attention.

M. Bourguignon, who has distinguished himself by many observations and investigations in regard to the subject of entomology, read an interesting paper before the Academy, at its last sitting, upon the itch of different animals. It had been generally believed that in each animal this disease was peculiar, and transmissible only to one of the same species. The acarus of man had been deposited upon dogs, cats, rabbits, birds, &c., without reproducing itself, or communicating the itch to those animals. And, on the other hand, the acari of horses, sheep, &c., when placed under the skin of certain medical students, and surrounded by all the circumstances most favourable to their

germination, had died almost instantaneously. M. Bourguignon discovered, by means of the microscope, a remarkable resemblance between the acari of man and those of the lion, and immediately proceeded to test the susceptibility of the one to the disease of the other. According to his account, the experiment was perfectly satisfactory, and it is now an established fact that an analogy exists in this particular between the king of the forest and the lord of creation. Thus has a most important service been rendered to pathology in general, and dermatology in particular, by the much abused and *fallacious* microscope.

A new remedy has recently been discovered for mercurial stomatitis, which is likely to prove a most valuable therapeutical agent. It is well known that many experiments have been made for the purpose of ascertaining what chemical changes can be effected in mercury taken into the system, so as to render it harmless in its effects, and to make its elimination expeditious and easy. For the purpose of disengaging it from its combinations with albumen, restoring its natural solubility, and facilitating its exit from the economy, many different substances have been administered, as the acetate of lead, preparations of sulphur, the sulphite of soda, &c. All who have had any experience in the practice of medicine, know that these remedies are not sufficiently certain and efficient in their effects to elicit much faith in their potency when the mercurial poison has really taken possession of the system. According to the experience of Messrs. Herpin and Blanch, of Geneva, the chlorate of potash, administered internally, answers the ends which have so long occupied the attention of the profession, and fulfils all the indications present in a case of mercurial stomatitis. They report a number of cases in which this remedy effected cures with marvellous certainty and rapidity. They say that it is equally sure in its effects upon adults and children, and that in every particular it is worthy the confidence and respect of the profession. It is administered in doses of 15 to 30 grains dissolved in a solution of gum acacia and water or some other vehicle of the same description. The whole quantity is taken in the space of twenty-four hours, if the urgency of the case demands it. The dose for children is, of course, smaller, following, in fact, the general rule established for other medicines.

An animated discussion came off at the last meetings of the Academy, between M. Bousquet and M. Piorry, in regard to the specific nature of disease. The latter read a long paper before the Society in which he took strong grounds against the nomenclature now employed by medical men, for the reason that it is based upon mistaken ideas of disease itself. He attempted to demonstrate an absolute non-identity between diseases, and to show that they are separable into a certain number of distinct organic morbid conditions. In fact, he seemed to look upon man, in his relation to pathology, as nothing more than the juxtaposition of a certain number of organs and members, between which there is no distinct and necessary connection or sympathy. This called up M. Bousquet, who made an animated and able reply. He attacked the foundations of M. Piorry's argument by showing that an intimate and necessary connection existed between the different component parts of the system, and the maladies which pertain to them. He ridiculed the changes proposed in the nomenclature of medicine as absurd and unnecessary in every particular, innovations which the necessities of science did not in the least degree demand. Unfortunately, however, he went too far. He declared that he placed the causes of all maladies in the source of life itself; that he was a *vitalist* in the fullest force of that term. This doctrine is more dangerous than the other, since it condemns therapeutics to an absolute sterility, closes the door against all further research in medicine, and prevents the

advance of the profession in its most important and practical direction. To place the root of all diseases in "vital action" is to transfer it to a *terra incognita*, which mortal ken cannot penetrate or explore. A French journal in giving an account of the discussion, closes its remarks with the following expressive exclamation: "O, vitalism! how many faults have been committed in thy name!" There is certainly much truth as well as force in the expression, and it should be inscribed upon the tombs of the advocates of this peculiar doctrine, as a warning to their followers.

M. Dubois has performed the Cæsarean operation twice within the last two months, without saving the mother in either case. After the last operation, he delivered a very interesting lecture to the class, giving his reasons in full for resorting to so extreme a measure, and stating the principles upon which he had acted under such circumstances during his life. He said that the inferior strait was too small to allow the passage of the child, either living or dead; that he knew the child was alive, and that experience demonstrated the *possibility* of saving the mother after the operation. In this state of the case, he had no alternative left but to resort to the Cæsarean section. As a general rule, he preferred cephalotripsy to the Cæsarean operation, for the reason that it was better to sacrifice a child which had formed no ties in the world than a woman who had already many complicated relations with society. He considered the question between the two operations to be one for the conscience of each individual physician, and about which no general rules could be established. He said that, among the poor, the chances for the life of a motherless child were very slight, and that as experience showed that the *probabilities* were against saving the woman after the Cæsarean section, it was his practice, particularly in hospitals, to perform cephalotripsy instead of the other operation. One could hardly have supposed that views so sensible and just, would have met with any opposition. They have, however, been warmly combated, in a recent number of the *Gazette des Hôpitaux*, by Dr. Liégard. He thinks that it is a question for science, not for conscience. He believes that the experience of the past has established a rule on the subject, which should be universal in its application. He declares it to be the duty of the physician to save the child, without taking into the account the right of either to life. He considers it better to sacrifice the mother, who has been *baptised*, than the child to whom circumstances have denied that *inestimable* right. These are his views on the subject, and I am afraid that they are entertained by but too many physicians of France. There is no telling to what extremities of folly blind superstition may drive even the most intelligent men, and when this *holy fear* of sacrificing both the *soul* and *body* if the child comes in to warp the judgment of the profession, it is to be apprehended that the learned professor may find himself in a small minority. Dr. Liégard gave some valuable statistics on the subject, which I will repeat.

Sounias, a physician of Bruges, reports that his own wife had submitted seven times to the Cæsarean section.

Simon has collected sixty instances in which the women had survived this operation. Tenon has reported seventy cases at Hôtel Dieu, which had presented themselves since those previously noticed by Caspar Bauhin, at the end of the 17th century; and Michel and Weideman have reported many successful cases.

Baudelocque, out of 111 Cæsarean operations, saved 48 patients.

In my next letter, I will report the progress of this discussion, as well as give a general sketch of the progress of medicine in Paris.

PARIS, *March*, 1855.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Abstract of Experiments upon the Physical Influences exerted by Living, Organic and Inorganic Membranes upon Chemical Substances, in Solution, passing through them by Endosmose. By JOSEPH JONES, A. B., Student of Medicine in the University of Pennsylvania. (With a Plate.) Read before the Academy of Natural Sciences, Philadelphia, Oct. 25, 1854. Communicated by the Author.

The phenomenon of endosmose is coeval with that of life. All organized beings spring from organic cells. Each cell is a chemical laboratory; the materials for its nutrition, growth and elaboration all pass through its walls by endosmose. What is true of the organic cell, is also true of every living organism, however simple or complex, because all its acts of nutrition, secretion and growth, are carried on by organic cells. These facts show the important bearing of endosmose upon physiology and pathology, and demonstrate the absolute necessity of studying and understanding its phenomena, and developing, as far as possible, all its physical laws.

A true knowledge of these can only be gained by careful, laborious, and numerous experiments.

Experiments upon Living Animals, showing that their Membranes exert a Physical Influence capable of changing the molecular arrangement of Chemical Substances in solution, passing through them by Endosmose.—I immersed the belly and lower extremities of a large spring-frog, in a strong solution of the chloride of calcium, and allowed it to remain for an hour; then washing it carefully with water, immersed its lower extremities in a strong solution of the oxalate of ammonia. It remained in this solution for one hour, when it was again carefully washed and placed in the solution of the chloride of calcium, where it remained for a similar length of time. Finally, it was removed from this, back to the solution of the oxalate of ammonia.

During the course of the experiment, which lasted four hours, the frog gradually became weaker and more inactive, and finally died in the solution of the oxalate of ammonia in which it was last placed. The specific gravity of the solution of the oxalate of ammonia was much less than that of the chloride of calcium, consequently the object in moving the frog from one solution to another, was to obtain as large a deposit of the oxalate of lime within its bloodvessels and tissues as possible, by promoting the actions of imbibition, endosmose, and exosmose.

As a general rule, the rapidity of the action of endosmose is proportional to the difference of the densities of the exterior and interior fluids. The effects of this action, or rather of the membranes, upon the chemical substances passing through, will be manifested by the change of physical form exhibited by the precipitate of the oxalate of lime.

Immediately after the frog had been removed from the solution of the oxalate of ammonia, an incision was made below and to the side of its sternum,

into the cavity of the thorax and abdomen. The heart was found still pulsating. The bloodvessels beneath the skin, upon the surface of the stomach and intestines, in the muscular fold of the peritoneum, and between the kidneys, appeared thoroughly injected with blood, as if the animal had died of violent inflammation of all these parts.

The blood from the heart and lungs was first examined under the microscope, in which, with a magnifying power of 210 diameters, were seen innumerable minute cubical particles, floating amongst the large elliptical blood corpuscles. In the fibrous tissue between the epidermis and pectoralis major muscle, square plates, cubical crystals, and parallelograms were seen, together with minute particles, the largest of which appeared to be cubes.

The mesenteric fold of the peritoneum contained the same equilateral, equiangular plates, and cubical crystals. In the fascia of the thigh, besides numbers of these, there appeared, also, beautiful octohedral crystals of the oxalate of lime, similar in all respects to those formed when the intestines of a raccoon (*Procyon lotor*) were filled with a solution of the chloride of calcium, and immersed in a solution of the oxalate of ammonia.

When the eggs of the frog were mashed, and their contents spread out on a glass slide and examined under the microscope, they contained multitudes of equi-angular plates, and cubical crystals of the oxalate of lime. It is probable that the exterior fluids passed through the anus and cloaca into the oviducts and ovaries, and finally by endosmose into the eggs themselves.

In the fibrous tissue of the walls of the abdomen, in addition to the cubes and octohedra, and equilateral plates, there appeared, also, delicately formed dumb-bell and ellipsoidal crystals.

When the plantar fascia of the foot was cut through, from the incision flowed a fluid resembling the liquor sanguinis mixed with a little blood, which, under a magnifying power of 210 diameters, contained beautiful octohedral and dumb-bell crystals of the oxalate of lime.

This experiment was repeated, with slight variations as to the length of the time and the density of the fluids, and in every instance, without any exception, the results were the same.

In one instance, the deposit formed within the blood and tissues of the frog assumed the form of delicate dumb-bell and cruciform crystals of the oxalate of lime. Fig. I. represents the appearance of these crystals and their relative size, when compared with the blood corpuscles. In another experiment the blood from the ventricle of the heart contained large and perfectly formed octohedra, with a few dumb-bell crystals, while the various tissues and muscles contained chiefly delicately formed dumb-bell crystals, and the aqueous humour of the eye contained octohedral, and comparatively large acicular crystals.

Fig. II. represents the crystals from the blood of the heart.

Fig. III. those found in the tissues, and between the fibres of the muscles.

Fig. IV. the acicular and octohedral crystals found in great numbers in the aqueous humour of the eye.

We see, then, by these microscopical examinations, that the oxalate of lime has assumed forms different in all respects from those of the precipitate thrown down, when solutions of the chloride of calcium and oxalate of ammonia are brought into immediate contact. The precipitate thus formed consists of minute irregular granules. Fig. V. represents this deposit. What has changed the physical form of this deposit? It has been accomplished, without doubt, by the action of the membranes upon the chemical substances in solution passing through them.

Experiments upon the Physical Influence exerted by Mucous Membranes, removed from all vitality, upon chemical substances in solution passing through them.—I filled the intestine of a raccoon (*Procyon lotor*) with a solution of the chloride of calcium, whose specific gravity was 1031, and immersed it in a solution of the oxalate of ammonia, having a specific gravity of 1007. In the course of an hour the exterior fluids became cloudy, with a white precipitate of the oxalate of lime. At the end of two days, a copious white deposit had settled to the bottom of the jar, which, under a magnifying power of 210 diameters, presented the appearance of innumerable acicular, rectangular, and irregular particles, often conglomerated together in great numbers, forming miniature representations of plants with their branches and leaves. Fig. VI. represents their appearance.

Specific gravity of exterior fluid 1005.

The interior fluid was next examined, the intestine having been punctured and its contents carefully removed.

Specific gravity of interior fluid 1003.

This marked change of its specific gravity from 1031 to 1003, shows that a free interchange of the fluids must have taken place. The slight change in the exterior fluid, of 1007 to 1005, is readily explained, when we consider the fact, that the exterior was 12, whilst the interior was only 4 fluidounces.

Within the intestines but a small deposit had taken place, in comparison with that of the exterior fluid. Under the microscope this presented a magnificent crystalline appearance, differing wholly from that of the exterior fluids, and also from that formed when solutions of the chlorides of calcium and oxalate of ammonia are brought into immediate contact. The appearance of this magnificent microscopical object is represented in Fig. VIII.

Amongst the crystals there were no less than seven well defined regularly formed varieties. We recognize the octohedral and dumb-bell crystals, as the form in which the oxalate of lime almost invariably occurs in the urine, not only of man, but also of other animals, and even in that of birds.

Does not this experiment indicate that the peculiar forms of the oxalate of lime, occurring in urine, may be the result of the physical action of the basement membrane of the tubuli uriniferi and its secretory cells?

The tissues of the intestine were next examined.

The cellular tissue was not equally injected; in some places there was scarcely any, while in others there was a very abundant deposit.

In all places the mucous membrane appeared free from any deposit of the oxalate of lime. It was found difficult, however, to decide this question by the microscope, on account of the difficulty of separating the fibrous tissue completely in which occurred a copious crystalline deposit.

This is not the only instance; out of numerous examples, we will select the following:—

When the bladder of a raccoon (*Procyon lotor*) was filled with a solution of the bichloride of mercury, and immersed in a solution of the iodide of potassium, a brilliant red crystalline deposit of the biniodide of mercury took place upon the exterior, whilst upon the interior a light yellow mass of lozenge-shaped crystals of the protiodide of mercury was precipitated. In this case also the mucous membrane appeared free from any deposit.

When the intestines of a raccoon were filled with a solution of the acetate of lead, and immersed in a solution of the bichromate of potassa, the deposit upon the exterior consisted of innumerable small irregular granules, while that upon the interior consisted of beautiful stellate crystals.

When a sheep's bladder was filled with a solution of the oxalate of ammo-

nia, and immersed in a solution of the chloride of calcium, no deposit took place in the exterior fluid, whilst a precipitate of the oxalate of lime fell in the interior fluid.

The appearance of this under a magnifying power of 210 diameters, is represented in Fig. X. Within the muscular and fibrous coats of the bladder this deposit presented the same appearance. The fact that solutions of certain chemical substances will pass through a membrane in one direction, but not in another, might be illustrated by many examples.

The stomach of a raccoon was filled with a solution of the bichromate of potassa, and immersed in a solution of the acetate of lead; a copious deposit of the chromate of lead took place in the exterior fluid, whilst none whatever occurred in the interior; it retained its natural color and appearance. The results were in all respects the same when the intestines of this animal were treated in a similar manner.

When the stomach was treated in this manner a deposit took place only upon the interior. When the œsophagus of a large rattlesnake (*Crotalus adamanteus*) was treated in a similar way, only a small deposit occurred in the exterior fluid.

I filled the stomach of a raccoon, and a portion of the intestines of a large rattlesnake with a solution of the iodide of potassium, and immersed them in a solution of the bichloride of mercury; in both cases a copious deposit occurred upon the exterior, whilst little or no precipitate fell in the interior fluid.

These facts are due to one of two causes. Either certain chemical substances in solution exert an influence upon mucous membranes, changing their minute anatomical structure, and thus destroying their power of carrying on the physical phenomena of endosmose and exosmose; or else mucous membranes possess a power of choice, as it were, dependent upon thoir physical constitution, allowing one fluid to pass through in one direction, but not another fluid holding a different chemical substance in solution in an opposite direction. When a portion of the small intestine of a sheep was filled with a solution of the nitrate of lime and immersed in a solution of the oxalate of ammonia, a copious precipitate of the oxalate of lime took place in the exterior fluid, which under a magnifying power of 210 diameters was found to consist of innumerable octohedral crystals of different sizes, also a few dumb-bell crystals. The appearance of this deposit is represented in Fig. VII.

In the exterior deposit the octohedra were about one hundred times more numerous than the dumb-bell crystals.

In the interior fluid a deposit of the oxalate of lime had taken place, which under the microscope presented the appearance represented in Fig. IX. The entire deposit consisted of innumerable minute and delicately formed dumb-bell crystals, with here and there an octohedral crystal. In the interior fluid, unlike the exterior, there were over a hundred dumb-bell crystals to one octohedral crystal. Minute octohedra, and delicately formed dumb-bells, were found within the meshes of the fibrous tissue. By comparing this experiment with the former ones in which solutions of the same chemical substances were used, we are forcibly taught the following laws:—

1. Mucous membranes from the same relative part of the bodies of different animals, exert different physical influences upon the same chemical substances.

2. Mucous membranes from the same animal, but from different parts of the body, exert different physical effects upon solutions of the same chemical substances.

Fig. I

Fig. II.

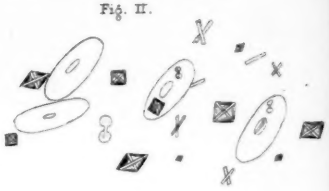


Fig. III.

Fig. VI.

Fig. VIII.

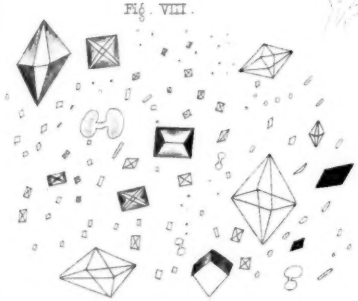


Fig. V.

Fig. IV.

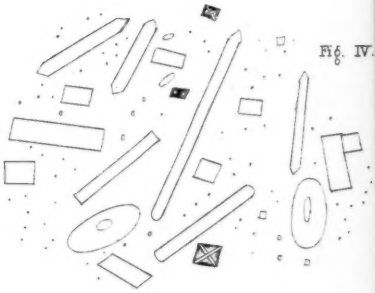


Fig. VII.

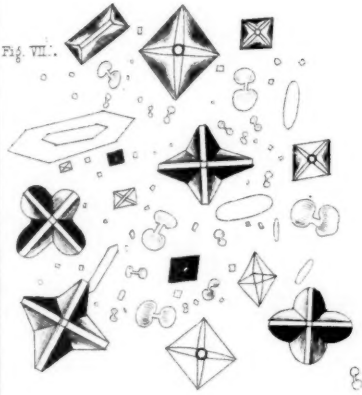


Fig. X.

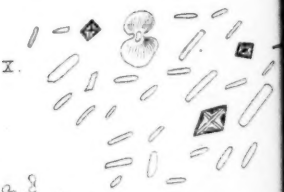


Fig. XI.

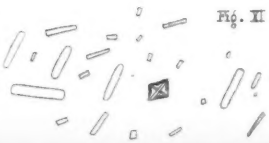
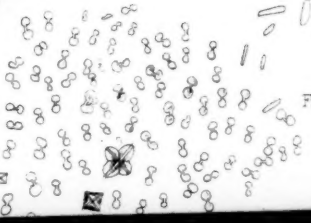


Fig. IX.



3. The physical influence exerted by the membrane is not the same in endosmose and exosmose; it differs with the direction of the current.

A very important question now presents itself for our consideration. May not this change of form in the precipitates be due to the presence of some animal substance or fluid, as fibrin, blood, albumen, or serum, and not to the physical action of the membranes? To determine this point a series of careful experiments were instituted with the following substances; albumen, fibrin, yolk of hen's egg, warm blood, cold blood, putrescent blood, warm serum, cold serum, putrescent serum, and urine. In no case did the presence of these substances produce a crystalline deposit of the oxalate of lime. Each experiment was performed in several different ways, and under different circumstances; sometimes the densities of the solutions of the chloride of calcium and oxalate of ammonia were varied, at others the temperature and conditions of the foreign body. But one result attended all these experiments—no crystalline deposit. In many instances we have mingled the interior and exterior fluids, in both of which crystals had been produced by the action of the membrane, and in every case the precipitate of the oxalate of lime thrown down consisted of irregular granules without any crystalline form whatever. Does not this prove conclusively that the simple presence of the different membranes did not cause the change of the physical form of the precipitate?

The next question which presented itself was, whether dry membranes exert a physical influence upon substances passing in solution through them, capable of changing their physical forms? To determine this point we performed several experiments with dry membranes, with solutions of the chloride of calcium and oxalate of ammonia, varying the relative positions and densities of the fluids in each experiment. In no instance was a regular crystalline deposit obtained. In only one experiment two or three octohedral crystals occurred in the midst of millions of irregular particles.

All our experiments, thus far, prove that dry membranes exert little or no physical influence upon chemical substances in solution passing through them.

Our next subject was to ascertain the influence of inorganic septa, during the endosmotic action. In our experiments we used thin vessels of baked clay. These were filled with a solution of the chloride of calcium and immersed in a glass jar containing a solution of the oxalate of ammonia. The relative positions and densities of these fluids were also changed. In no instance did we obtain a regular crystalline deposit. So far, then, as our experiments go, we are able to assert, that inorganic septa do not exert a physical influence upon chemical substances passing through them, capable of changing the arrangement of their molecules.

Experiment upon the Physical Influence exerted upon Chemical Substances in solution, as they pass through the cell walls of vegetables.—I immersed the cut end of a stalk of Indian corn in a solution of the chloride of calcium, and allowed it to remain for 18 hours, at the end of which time it was removed and placed in a solution of the oxalate of ammonia, in which it remained for a similar length of time. When thin slices of the corn-stalk were examined under a magnifying power of 210 diameters, a crystalline deposit of the oxalate of lime was found to have taken place within the hexagonal cells of the vegetable, which presented the appearance represented in Fig. XI. This differs widely from the deposit formed when solutions of the oxalate of ammonia and chloride of calcium are brought into immediate contact. The precipitate thus formed consisted of irregular granules.

The crystals deposited within the cells of the corn, differed widely also from those formed when the intestines of a raccoon or a sheep were filled

with solutions of the oxalate of ammonia, and immersed in solutions of the chloride of calcium.

I immersed sections of different lengths of the stem of a young and verdant benne plant, in a solution of the acetate of lead, and then transferred them to a strong solution of the proto-sulphate of iron. When thin sections were examined under the microscope, beautiful square and lozenge-shaped plates had crystallized in all the hexagonal cells.

When the broad thick leaf of an endogenous plant was placed alternately in solutions of the same chemical substances, a crystalline deposit took place within its cells, which differed in form from that within the cells of the benne plant. Different vegetables were immersed in different chemical solutions, which, when mingled, produced a deposit of irregular granules, and in every instance a regularly crystalline deposit took place within their cells.

It is unnecessary to do more than recapitulate the following results:—

1. Cell-walls, like animal membranes, exert a physical influence upon the chemical substances held in solution passing through them. This physical influence is capable of altering the arrangement of the molecules of the precipitate formed within the cells, so that the precipitate which under ordinary circumstances consists of irregular granules, under the influence of the endosmotic action assumes a regular crystalline form.

2. The cells of different vegetables, like different animal membranes, change in different manners the arrangement of the molecules of the same substance.

It may yet be demonstrated, by experiment, that cells in the same plant, having different offices, elaborating different products, exert a different physical influence upon the same chemical substance. Or, in other words, the crystalline deposit of the same substance, will vary in physical properties with different cells.

In conclusion, we would state that we are still investigating this subject, and hope to be able to substantiate, by numerous experiments, performed under every possible circumstance, all the laws and facts asserted in this brief abstract of experiments.

—

Case of Quadruple Birth.—By Wm. Rankin, M. D., of Shippensburg.—*April* 20, 1854. I was called to visit the wife of Mr. John Tarvin, farmer, who is supposed to be in labour, having completed the full term of her fourth pregnancy. On my arrival, I found a child delivered; but on examination, ascertained that there was another contained in the uterus. After waiting nearly an hour from the time the first child was born, for further uterine contractions, I ruptured a set of membranes that presented in the form of a pouch at the os uteri, when a pain came on and delivered another child. On examination over the region of the uterus, I discovered the presence of another child; and no pain coming on, after a reasonable delay, I ruptured a third set of membranes, when a powerfully expulsive contraction took place and expelled a third child. From the usual examination, I ascertained that there was yet another child to be delivered; and no pains coming on, I resorted to the plan of rupturing the membranes, when almost immediately a pain occurred and effected the delivery of a fourth child. By a little manipulation over the region of the uterus it contracted and expelled from its cavity two placentæ, to one of which three cords were attached, and to the other but one; the whole mass not larger, I think, than I have often seen after the delivery of one child. The cords were something smaller than the average size. The uterus afterwards contracted well, and there was no more hemorrhage than takes place after the majority of cases of parturition. The woman appeared very much exhausted and overcome by distracting emo-

tions in consequence of the enormous brood of children to which she had just given birth; but after encouraging and consoling her, as well as I could, by pointing out her duty in the case and assuring her that there would, undoubtedly, be ample means provided for their maintenance, she soon assumed a cheerful countenance, her general debility gradually abated, and she progressed through her confinement as favourably as most women do after giving birth to a single child.

The first and last born were rather larger than their mates, and were all apparently healthy and viable, and, according to their mother's reckoning, had come to their full time. Although I unfortunately neglected weighing them, I think they would have averaged four or five pounds.

They were all of the male sex. The hair nearly red, and the complexion florid in all of them.

The mother is a woman of slender and rather delicate form, of medium height, and generally healthy, of about thirty-five years old; and, before this quadruple birth, had had three daughters at as many births; five years have elapsed since the last. Her complexion and hair are dark. The father is a stout, athletic, healthy man, of about thirty-eight years of age; hair fair and complexion florid; of sanguine temperament.

The first born of the children, about eight weeks after birth, sickened and died after a few hours' illness, most probably from the almost unavoidable irregularity in giving it nourishment.

After the milk was fully secreted the mother had, for a month or so, plenty of nourishment for them all, after which time food was required from other sources, principally cow's milk diluted with water and sweetened with loaf sugar. The three surviving are now, 14th of January, 1855, quite healthy and thriving children.

—

Paste of Allium Cepa as a remedy for Epistaxis.—Benj. Rorer, M. D., of Germantown, has favoured us with a communication in which he extols the efficacy of a paste made of the *Allium cepa* in arresting atonic hemorrhage from the Schneiderian membrane.

The directions for making the paste are as follows:—

"Take the inner portions of the bulb of the *A. cepa* or common onion, cut it fine, mix it with an equal portion of flour or bread crumbs, adding a sufficient quantity of strong vinegar to make a consistent paste." This paste is applied by pressing it into the nostril with the ball of the thumb until no more can be introduced, when the plug is to be secured by a bandage. Several cases are detailed illustrative of its efficacy, one of which we shall quote.

G. F., aged 44 years, a baker, was attacked, March 6th, 1847, with epistaxis without any premonition, while attending to his heated oven, and had lost, he stated, 10 or 12 ounces of blood before Dr. Rorer saw him. Dr. R. had him placed in a cool room, washed his face and head with iced water, and injected the same into his nares, and afterwards solutions of alum and of the metallic salts. Alum and finely powdered galls were then applied by means of a quill, and lastly plugging with lint wet with astringent solutions resorted to. At the same time 20 drops of muriated tincture of iron were given every two hours, all without any avail. At this time a German named Seibert advised the onion paste, which was used with the effect of completely arresting the hemorrhage.

—

Blighted Fœtus at the fifth month retained and expelled with a Living Child at Full Term. By Wm. M. Belt, M. D., of Independence, Mo.—I was called on the 23d day of January, 1852, to see a girl in the employ of

Hiram Young, of this place, supposed to be threatened with abortion. She stated that the waters had escaped; there was slight hemorrhage and strong uterine pains. I kept her in a recumbent position, and gave a mild cathartic. In five or six days she was able to walk about the house, and soon recovered her usual health. On the 21st day of May following, I delivered her of a healthy child. In tracking the cord for the placenta, I came in contact with a *dead fœtus* of five months.

INDEPENDENCE, MO., *Jan.* 1, 1855.

DOMESTIC SUMMARY.

Peritonitis in Typhoid Fever, without Intestinal Perforation.—The following very interesting case was read to the College of Physicians of Philadelphia, by Prof. G. B. WOOD.

"J. B., aged seventeen years, entered the Pennsylvania Hospital on the 2d of January. According to his own account, he had been three days ill; but the symptoms, which were those of typhoid fever in the middle of its course, evinced that he was in the second week of the disease. The tongue was quite dry, and the abdomen tympanitic; and the characteristic red spots, if not present at the time of his entrance, made their appearance very soon afterwards. Under the plan of treatment usually pursued in the hospital, including the use of oil of turpentine as an alterative to the ulcerated surfaces in the intestinal mucous membrane, he gradually amended; and, on the 20th, I presented him to the class in attendance upon the clinical lectures as quite convalescent. On the 21st, however, he was suddenly seized with severe abdominal pains, tenderness on pressure, and great prostration; and it was evident that he was labouring under an attack of violent peritonitis. I had no doubt of the existence of perforation of the bowel, which I felt disposed to ascribe to an orange which he had surreptitiously eaten a few days previously, and of which, in order to conceal his violation of the rules of the ward, he had probably swallowed the seeds and rind. With this conviction of the nature of the case, I directed the use of sulphate of morphia largely, according to the well-known plan of Drs. Graves and Stokes, with perfect rest, a blister over the abdomen, and, towards the close of the case, stimulants in order to support life. The system never fully reacted, and death took place on the 25th.

"Dr. Forbes, one of the resident physicians of the hospital, opened the body, and found a small quantity of turbid liquid in the abdominal cavity, with evidences of almost universal inflammation of the peritoneum; but, on the closest and most careful inspection, could discover no perforation of the bowel; nor was there the slightest fecal odor in the liquid effused. I had afterwards an opportunity of examining the whole of the intestines, though not *in situ*. The internal surface of the ileum presented several recent and well-defined *cicatrices*, finely marking the boundary of the patches of Peyer's glands, which I exhibited to the class as the finest example I had seen of this appearance; but there was not a single remaining ulcer visible, and I could find no sign of an opening. The peritoneal coating was covered with a very thin, glutinous, and translucent exudation of coagulable lymph, but with little redness. At no one point upon the surface were there more decided marks of inflammation than elsewhere, to indicate the possible seat of an opening. I was compelled, therefore, to regard the case as one of peritonitis without perforation. No discoverable cause of the affection existed; and I am inclined to rank this with those not very uncommon cases, in which peritonitis comes on without any special known cause, at the close of long-continued and exhausting affections, perhaps in consequence of depravation of the blood.

"This case is calculated to throw great doubt upon the existence of intestinal perforation in those instances of peritonitis, occurring in the advanced

stage of typhoid fever, in which cures have been effected, of which I have been so fortunate as to witness two, in my own experience. They may have been, as in the case just related, nothing more than simple peritonitis without any opening whatever through the coats of the bowel. One important practical inference, however, may be deduced from them, viz: that the opiate treatment is the one best adapted to peritonitis occurring under these circumstances, whether with or without perforation; as several instances of recovery have taken place under that treatment, while I am not aware that one is on record effected under any other plan."—*Summary of Transactions Coll. Phys. Phil.*, Vol. II., N. S., No. VII.

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A Grain of Coffee in the Air-Passages—fatal after being retained nearly three years and a half.—At the Meeting of the College of Physicians of Philadelphia, Nov. 1, 1854, Dr. Wm. Pepper related the following interesting case:—

On the 22d March, 1844, a little girl, aged eighteen months, whilst playing on the floor, was suddenly seized with a violent spasmodic cough, threatening strangulation. The mother recollected that the child had been amusing itself with an uncooked grain of coffee, and immediately suspected that it had been swallowed and was the cause of all the difficulty. The room was carefully searched, but the grain could not be found; and this but confirmed the mother's conviction. Dr. Maris saw the child an hour or two after the accident; she was then in a disturbed sleep, with hurried breathing and suffused face; in a few moments she was aroused by a return of the same agonizing cough and spasm. Various expectorants were now administered, but without relief; and the remissions and exacerbations of the cough continued to recur every few minutes.

After an interval of about six hours, I was requested to see the child; but found her so much oppressed and restless, that it was almost impossible to auscult with any degree of accuracy. The respiratory murmur on the right side was exceedingly feeble, and, in a great measure, masked by loud sonorous râles, whilst on the opposite side the murmur was unusually loud; throughout the entire chest, the percussion was perfectly natural. The history of the case, confirmed by the physical signs, left but little doubt as to the nature of the accident, and tracheotomy was accordingly proposed. This, however, was rejected by the father, under the impression that the symptoms might be owing to some other cause, and in the hope that, even if our fears were well founded, the coffee-grain might be spontaneously expelled. Upon several occasions, when the violence of the cough indicated that the grain had been driven into the larynx, the child was inverted, in the hope that gravity might assist in its expulsion; but this expedient only tended to increase the alarming symptoms, and was therefore abandoned. From this time, I did not see her until the early part of August, 1847, a period of more than three years. Dr. Maris, however, informed me that, a few weeks after the accident, the paroxysms of cough gradually became less violent and of shorter duration, whilst the intervals were much longer, lasting sometimes for several weeks, during which the child was comparatively comfortable, and gained both flesh and strength, but never entirely free from difficulty of breathing. Finally, hectic fever and purulent expectoration set in, and she became greatly emaciated. Upon examination, I now found dulness on percussion over the lower part of both lungs, but particularly on the right side, and the respiratory murmur throughout was completely masked by loose mucous râles; the prostration, however, was so great as to preclude the possibility of a more extended examination. From this time she gradually sank, and finally became comatose, and died on the 24th of the month—just two weeks after my last visit, and about three years and five months from the time of the accident.

The body was examined the following day. Upon opening the thorax, care was taken to remove both lungs, with the trachea, larynx, and œsophagus adherent, without incising these parts; gentle pressure was now made with the finger along the course of the trachea, from below upwards, until the larynx was reached, when the grain of coffee was seen to escape from the chink of the glottis. It, apparently, had undergone no important change from its long

stay in the air-passages: it had not materially increased in bulk from absorption of moisture, as might have been expected, nor was it even coated with pus, mucus, or other animal matter, and its usual colour was well preserved; upon the application of considerable force, it could, however, be slightly flexed. The mucous membrane of the larynx and trachea was thickened, minutely injected, and coated throughout with tenacious muco-puriform matter; and this condition of things extended throughout the bronchial tubes, as far as examined. At the bifurcation of right bronchus, the membrane presented a worm-eaten appearance, and was much softened, though not absolutely ulcerated. The entire right lung was hepatized, and about one-half of the left lung, at its lower part, was in a similar condition. No abscess or tubercular degeneration existed in either lung, nor were there any pleuritic adhesions.

The condition of the right bronchus, at its bifurcation, renders it highly probable that the foreign body was lodged the greater part of the time at this point. The chronic inflammation of the left bronchus and lung may have been owing to an extension of the inflammation from the right side, where it undoubtedly first commenced; or it may have been induced by the occasional lodgement of the coffee-grain in the left bronchus, after having been driven into the trachea or larynx. Judging from the size of the foreign body, compared with the caliber of the bronchus at the tender age at which the accident happened, it is highly probable that the air must at the first have been almost entirely excluded from the right lung; and this must have been still more the case when lodged in the left bronchus, owing to its comparatively small size. It has been admitted by all observers, that when the foreign body is irregular in shape it is more apt to become permanently fixed in the bronchial tubes, and at the same time to interfere less with the respiratory movement, than when it is round or oblong, and thus completely plugs up the tube, as was the case in the present instance; and this fact fully explains the great fluctuations in the symptoms.—*Quarterly Summary Trans. Coll. Phys. of Phila.*, Vol. II., N. S., No. VII.

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Achillæa Millefolium in Hemorrhage.—Dr. B. H. Coates stated, at the meeting of the College of Physicians of Philadelphia, on Dec. 6, 1854, that he had used an infusion of *achillæa millefolium*, with material advantage, in several cases of hemorrhage; two from the kidneys, three (in the same subject) from the uterus, and one from the lungs. The formula was half an ounce of the herb to a pint of hot water; to digest for twenty minutes. The dose of this infusion to be taken cold is half a teacupful every two hours.—*Summary of Trans. Coll. Phys. Phil.*, Vol. II., N. S., No. VII.

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Erigeron Philadelphicum in Uterine Hemorrhage.—Dr. Wilson read to the Philadelphia College of Physicians, Nov. 1, 1854, notes of some cases of uterine hemorrhage successfully treated by the essential oil of *Erigeron Philadelphicum*. Dr. Wilson, whose attention was drawn to this article by Dr. A. C. Bournonville, who had used it in some few cases with advantage, states that he has prescribed it in many cases, and very generally with decided advantage. He prescribes it in doses of five drops every two hours.—*Quarterly Summary Trans. Coll. Phys. Phil.*, Vol II., N. S., No. VII.

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Ligature of the Gluteal Artery.—Dr. Warren Stone has lately performed this operation in the Charity Hospital, on a boy, ten years of age, who had a large pulsating tumour in the gluteal region, following a wound by a narrow bladed knife, on the 28th December, 1854. The operation was performed on the 7th of February last, in the following manner: Dr. Stone made an incision across the top of the tumour, following the course of the fibres of the gluteus major muscle, eight inches in length, scooped out the coagulated blood, the sack being freely opened, and placed his finger upon the artery to control hemorrhage; a ligature was then placed around the artery, and a compress of lint gently applied to stop a slight oozing which existed from the neighbouring parts. The wound was drawn together by adhesive straps, an opiate was given,

and the patient put to bed. On the 24th February the patient was discharged, the wound being nearly healed.

Dr. Stone states that this is the third case of this lesion which has come under his care.

"The first case was brought to the Charity Hospital in 1835, considerably exhausted by the loss of blood, from a wound of this artery; it was treated by compression. The wound healed; but, about two weeks after, a small tumour was discovered in the region of the gluteal artery, which, upon auscultation, gave the usual aneurismal murmur. The tumour seemed dense, as if considerable lymph had been thrown out and organized for its protection, and pulsation was only observed when its walls were compressed. This patient remained in the Hospital about six weeks, and as the tumour did not increase, he was discharged; being warned of his condition and advised to return if the swelling should increase; but I heard no more of him. The second case was admitted into the Hospital in 1853, completely exhausted from the loss of blood. The wound was made six days previous to my visit to him, and he had suffered repeated bleedings. At the time I first saw him, he appeared perfectly bloodless, and nearly pulseless, having just had a hemorrhage, and it was thought that he could not bear the operation. Stimulants were administered, and in about an hour the pulse became steady; and as the operation offered the only possible chance for life, he was placed upon the operating table, and the artery tied. The operation was made by making an incision from near the posterior superior spine of the ischium, to the upper part of the trochanter major (which, from the distention of the parts by the coagulum beneath the muscle, was near a foot in length), the coagulum was scooped out, and the finger placed upon the artery just where it emerges from the pelvis. In the upper part of the ischiatic notch, it was found that the artery was cut very near to the finger, and it was with great difficulty that a ligature was placed upon it; this was finally done, and the patient was carefully stimulated and nourished; but, in spite of the greatest care, gangrene took place in the wound, in consequence of the great loss of blood, and he died on the fourth day after the operation.

"By most surgeons," Dr. Stone remarks, "it is considered sufficient to tie the artery on the proximal side of the wound; but Mr. Guthrie insists that it is important to tie the artery on both sides of the wound, at least when it occurs in the arteries of the extremities, as the free anastomosis, he asserts, renders hemorrhage very certain from the distal extremity. I have tied most of the large arteries for wounds, and in most cases have applied a simple ligature at a convenient place on the proximal side of the wound, and have never been troubled with hemorrhage from the distal orifice, except in one case. This was in a wound of the plantar arch, in a case that was admitted into the Hospital sixteen days after it was received. The patient, a little boy of ten years, was completely exhausted from repeated hemorrhages, and I tied the two tibial arteries, the anterior one on the dorsum of the foot, and the posterior one behind the inner malleolus. Three days after, he bled again, but not very freely, and it was thought a little pressure would command it; but the next day it recurred with more violence, and the patient was completely exhausted, and evidently could not survive any further loss of blood. The foot was swollen and painful, and it seemed impossible to secure the artery in the sole of the foot, without destroying the organ, and I determined upon tying the femoral artery, which was done without the use of chloroform as the patient lay in his bed, for he was so weak that I feared to disturb him; this secured the bleeding, and the little patient recovered perfectly.

"But notwithstanding the confidence which my experience gives me in the safety of tying the artery at a convenient place on the proximal side of the wound, I would advise, in cases of false aneurism, when there is considerable extravasation of blood, to lay the sack open freely, and tie the vessel at the wound, whenever it can be done without cutting through important parts. The impression is, that a large sac or cavity, containing blood, laid open and exposed to the air, will take on ill-conditioned inflammation. This is generally true, where the extravasation is from contusion, but there is a great dissimilarity in the cases. In the former case, the blood is isolated, and can be scooped out

clean, and the cavity heals kindly; but in the latter case, the blood mingles and clings to the contused parts, and cannot be sundered, and when exposed to the air decomposes and becomes a poison. Another fear that deters from opening the tumour is, that the artery may not be readily found, and the patient suffer from loss of blood. When the artery can be commanded by pressure on the proximal side, there can be no objection, and the bleeding mouth of the wounded vessel is readily discovered. The flesh is pushed away by the pressure of the extravasated blood, and leaves the mouth of the vessel isolated, compared with its position in a fresh open wound. The advantage of this method of operating is, that we get rid of the mass of blood, which will generally have to be discharged sooner or later by a separate opening, when the quantity is large, and we make the operation more certain, for the sac may be fed when the anastomosis is free.—*New Orleans Hospital Gazette*, March, 1855.

Case of Spontaneous Rupture of the Bladder.—Extravasation of Urine.—Operation.—Recovery. Dr. A. V. Williams, late Physician-in-chief to Ward's Island Hospital, records (*New York Medical Times*, Jan. 1855) the following extremely remarkable case.

A man, aged 32 years, of spare habit, but of great endurance, has for several years laboured under stricture of the urethra, with frequent desire to urinate from irritability of the bladder. He states that on several occasions, he has been unable to pass any water for several hours. On the 9th day of June, 1854, I was called to see him, when he stated that he had not passed any urine for two days; that this morning, when making a violent effort to do so, he felt something give way in his belly, and "felt a snap;" since which he has felt no desire to urinate, but the pain is very great over the belly. I endeavoured to pass a catheter into the bladder, but found it impossible to do so. He was put in a warm bath, a large anodyne administered, and a warm poultice applied over the abdomen.

June 10. He slept some last night, but otherwise remains in the same state, except that the abdomen was not swollen. I could not make any other *diagnosis* than that some portion of the bladder had given way when he felt the "snap;" and that the effusion of urine into the cavity of the abdomen accounted for the subsidence of the desire to make water, and also caused the other symptoms.

I requested Dr. Willard Parker to meet me, who, on examination, concurred with me as to the nature of the case, and of its desperate character.

On consultation, we agreed that the only chance for the man (and that a very small one), was to open the abdomen above the pubis, cut into the bladder, and pass a catheter, if we could, from within out through the penis, and re-establish a passage in that way. To cut into the bladder from the perineum was useless, as the effusion was above the pelvic fascia; and a puncture through the rectum was impracticable, as the bladder was empty, and could not be felt.

The case was fully stated to the patient, who requested me to "cut away."

After shaving the pubis, I made an incision about four inches long, from the pubis upward in a line with the linea alba, dividing the fibres of the pyramidalis; then very carefully divided the tendon beneath, and with a director dilated it freely. The urine flowed out abundantly from the wound. There was but little hemorrhage. The bladder was deep, and firmly contracted behind the pubis, and so altered in appearance that it could not be recognized as that organ. I pushed up the perineum with one finger, whilst Dr. Parker, with a hook drawing up the bladder, I punctured it with a bistoury; on dilating this opening with the finger, the internal surface was found corrugated and thickened. The opening into the urethra could not be felt, so that the original design of forcing a passage from within outwards, could not be done. Whilst my finger was retained in the bladder, Dr. Parker accordingly passed a grooved sound into the penis, down to the strictured part, and forced it forward until the point was felt by my finger through the thickened coats of the bladder, which were so tough I could not tear it with the nail, but cut upon the end of the sound, with a probe-pointed bistoury, carefully passed along the finger. The wound made in the abdomen was brought together by a single

suture; a catheter introduced through the false passage made into the bladder, a large anodyne given to the patient, and he placed in bed as soon as the operation was completed; he felt greatly relieved, and soon fell asleep.

11*th*. The patient says he feels pretty comfortable; he only complains of the general soreness of the belly. The urine flows from the wound, and also from the catheter.

12*th*. Had a bad night; pulse, 130; abdomen tender; urine flows as yesterday; bowels not opened; vomits a green matter; skin hot; but says "he shall get well," which opinion he stuck to throughout. Gave him opium grs. iij, with calomel grs. viij, and directed a pill of calomel gr. ij and opium ¼ gr. to be given every two, three, or four hours, according as he is restless; diet accordingly, with poultice to abdomen.

12*th*. Passed a more comfortable night; the vomiting has ceased; bowels freely purged; pulse, 130; abdomen tender; wound healing above the suture, whilst the urine flows from the lower part above the pubis, and through the catheter; continue the calomel and opium every four hours. The constant dribbling of the urine over the scrotum has produced excoriation, which annoys the patient more than his other troubles. This was relieved by simple cerate, and the parts protected from the urine by an oiled-silk bag.

13*th*. Patient attacked with violent hiccough, from which he suffers dreadfully. This symptom caused great distress, and continued without intermission for an entire week. Nothing seemed to do any good, and all remedies for it were abandoned, except a pill of opium three or four times a day; the calomel was omitted on account of his gums being sore. The symptoms of peritonitis are subsiding.

21*st*. The subsidence of the hiccough was a great relief. The patient began to show his sufferings. Beef-tea and milk punch were ordered, and a general supporting regimen was followed throughout the remainder of the treatment, with quinine.

A slough came out of the wound above the pubis, about the size of a pullet's egg, leaving a clean, granulating sore, which healed kindly. On the 27th day after the operation, the wound had entirely closed, and the urine passed through the penis in a fuller stream than it has done for years. The patient entirely recovered, and engaged in his labouring business as a journeyman butcher.

Dr. Cammann's Self-Adjusting Stethoscope.—Dr. Geo. P. Cammann, of New York, has invented a self-adjusting stethoscope, which consists of an objective end made of ebony, the extremity of which is about two inches in diameter, two tubes composed of gum elastic and metallic wire, two metallic tubes of German silver, two ivory knobs at the aural extremity, and a movable elastic spring, so arranged as to adjust it, and keep it in its proper position.

M. Landouzy, of Paris, previous to 1850, formed a stethoscope having a number of gum-elastic tubes, by means of which several persons could listen at the same time. Dr. N. B. Marsh, of Cincinnati, in 1851, patented a stethoscope with two gum-elastic tubes, and a membrane over its objective end. Dr. Cammann does not, therefore, claim any originality on account of the two branches of his instrument, but on account of other advantages which it possesses.

The bell-like expansion of the objective extremity has a convolvulous excavation, gently curving outwards to present a rounded edge to the chest, in order to prevent causing pain to the patient. The bore of the instrument is two and a half lines in diameter, care being taken to have it made smooth and even. The tubes are made of German silver, with a double curve towards the aural extremities, which curves require to be constructed with great care, so that the ivory knobs may rest closely upon the external openings of the ears. When applied, it is necessary that the orifices of the knobs should point upwards. Some of the instruments are constructed with a spiral, and others with an elastic spring, as shown by the plate. Some of them are so arranged that they can be disjointed, to render them more portable.

One point, heretofore *sub judice*, is settled by this instrument, viz: that the

sound is conducted entirely through the air, and not at all through the media, as these were, for experiment's sake, changed nine or more times, without affecting in the least the intensity of the conducted sound. On making the objective end solid, all sound was lost.

The advantages claimed for the instrument of Dr. Cammann are:—

1. That being applied, it adjusts itself closely to both ears, excluding all external sound.

2. It leaves both hands of the examiner free.

3. It gives sounds pure, and greatly increased in intensity, though differing in quality from those hitherto afforded by auscultation, the pitch being lower. This intensity is produced by both ears being acted upon at once, by the earpieces of the instrument fitting closely into the meatus of both ears, and by the smoothness and careful construction of the bore of the stethoscope as to curves, &c., according to the law of reflected sound.

4. Sounds not heard through the instrument in common use can be detected by this.

5. Sounds which are doubtful by ordinary instruments are made perfectly certain. Even when disease is seated in the central part of the lungs, they can be detected, when the ordinary stethoscope will fail to render them recognizable. The same advantages are obtained in examining the morbid sounds of the heart.

The great increase of intensity of sound by this instrument renders it valuable to those with impaired hearing.

In the use of this stethoscope it is necessary that the chest should be uncovered, to prevent all friction between it and the clothes; otherwise the sound thus generated is conducted with such intensity as to embarrass the examiner. A short practice may be required to become familiar with it, in consequence of the increased intensity of the sounds produced by it, and the difference between them and those afforded by ordinary auscultation. Many of the recognized physical signs of thoracic disease will be so modified as to be new to the examiner, but a short experience will enable him to appreciate them, and give them their true value.

These stethoscopes are manufactured and sold by Messrs. George Tieman & Co., No. 63 Chatham street, who pay particular attention to their construction—a point very essential to an instrument of this kind.—*New York Med. Times*, Jan. 1855.

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Reduction of Dislocation of the Femur by Manipulation alone.—Dr. Thomas M. Markoe, one of the Surgeons of the New York Hospital, has published (*New York Journal of Med.*, Jan. 1855) an account of the cases of dislocation of the femur at the hip-joint, treated by manipulation alone, during the past two years in the hospital to which he is attached, with some very interesting remarks on this method of reduction.

Our readers are aware, that Dr. Wm. W. Reid, of Rochester, N. Y., published in the Transactions of the Medical Society of the State of New York, for 1852, a paper on "dislocation of the femur on the dorsum ilii, reducible without pulleys or any other mechanical power," in which it is the object of the writer to show that this displacement can be remedied "by flexing the leg on the thigh, carrying the thigh over the sound one, upward over the pelvis, as high as the umbilicus, and then by abducting and rotating it."

"The fact," Dr. Markoe observes, "that luxations of the hip-joint might be reduced by manipulations, not requiring forcible extension by pulleys or other mechanical contrivance, seems to have presented itself to the mind of several writers both of ancient and of modern times. The idea would appear sometimes to have been suggested by accident, and sometimes to have been the result of reasoning on the mechanism of the joint and its displacements. Such a suggestive accidental reduction occurred in the hands of Dr. Physick, and is related in Dorsey's Surgery. A patient was the subject "of dislocation of the femur directly backwards, and after very powerful efforts had been made to effect the reduction by extension, Prof. Physick, conjecturing that the head might be confined in a slit in the capsular ligament, discontinued the extend-

ing force entirely, and then, with no more force than that of his own hands, abducted the thigh, when the bone instantly slipped into its place."

"A similar occurrence took place in our hospital, about the time of the introduction of the use of chloroform, in a patient with dislocated hip, under the care of Dr. Jno. C. Cheesman. While the preparations for applying mechanical force were being made, Dr. C. made some preliminary movements of the joint, while the patient was fully under the influence of chloroform, and, unexpectedly, the head of the bone slipped into its proper place. Dr. Nathan Smith, Prof. of Surgery in Yale College, succeeded in reducing a luxated femur, by manipulating, with the thigh and leg used as a lever, after the usual plan had failed. It would seem from the report of this case, which, however, is made from memory by his son, Dr. Nathan R. Smith, that it was a deliberate and prearranged plan of procedure, though the considerations which led to its adoption are not mentioned in the record.

"Chelius, in his work on Surgery, gives an outline of the views of four writers, who have proposed to effect the reduction of the dislocated hip by the hands alone of the surgeon and his assistants, and without the aid of pulleys, or, indeed, of any forcible extension. Three of these gentlemen, Wattman, Kluge, and Rust, have described methods not very different from each other, which essentially consist of moderate extension with the hand, combined with movements of rotation of the axis of the limb, adduction, flexion, etc., but with the addition of force applied by a band passed around the upper part of the limb, which is committed to an assistant, and, in the different forms of dislocation, is so applied as to lift the head of the bone from its unnatural resting-place, while the other movements made with the shaft of the limb give an opportunity to the muscles, stretched by the luxation, to draw the head back into the acetabulum. Another surgeon, referred to by Chelius, Colombat, speaks of a method which he has always employed with success, in which the patient stands at a table leaning forward with his chest upon it, and the surgeon, standing behind, bends the knee with one hand, and makes the various movements of the hip-joint, while, with the other hand on the ham, he makes gradual extension by pulling downwards. By this manœuvre, he states that he dislodges the head of the bone, and without any force accomplishes its reduction. In Casper's Wochenschrift for Nov., 1849, there is an account given by Dr. Fischer, of Cologne, of his mode of reducing luxations of the hip, which consists in flexing the femur on the trunk, and then making rotation of the limb, conjoined with adduction, or abduction, as the head of the bone is on one side or the other of the acetabulum. In the *N. Y. Journal of Medicine* for March, 1852, a Dr. Mayr is spoken of as having made use of this procedure of Dr. Fischer, without being aware of Dr. F.'s published statement."

Dr. Reid's observations appear to have been made independently of any knowledge of the previous opinions of others. In his paper he describes the steps by which he was led to the adoption of his mode of operation, and gives five cases of his successful application in practice.

It would thus appear, as remarked by Dr. Markoe, that the barren honour of being the first suggester of the idea of this method does not belong to Dr. Reid, but he accords to him "the higher honour of having caused the idea to assume the shape and value of a fact, and out of the bare suggestion to have, by patient, ingenious, and long-continued investigation and experiment, deduced an available, and, as we think, an exceedingly valuable addition to the resources of Practical Surgery."

Dr. Markoe gives the details of thirteen cases of dislocation of the femur, in all of which, with two exceptions, Dr. Reid's plan was successful.

"With regard to the rationale of the process," Dr. Markoe observes, "most of those who have written on this matter are in the main points agreed. The head of a dislocated femur is retained in its new position by a mechanism which does not exist in any other joint, and which is produced by the fact of the muscles not being inserted into the head, but into the trochanter, nearly three inches from the head, and that from this point of principal muscular insertion the neck goes off at a large angle from the axis of the shaft. From this, it happens that when the head of the femur is thrown out of its socket, the

trochanter no longer stands out more prominent than before, but being held firmly by the muscles which are inserted into its base, it is prevented from rising any more than enough to let the head out of the acetabulum, while the head and neck, slipping to the one side or the other, are found lying in such a manner that the side of the head, neck and trochanter are in contact with some part of the outer surface of the pelvis, varying, of course, in the different forms of luxation. This being borne in mind, it will be clear that any attempt at reduction, which merely brings the head of the bone to the acetabulum, will not succeed in making it enter that cavity, because of the lying-down position of the neck and trochanter against the side of the pelvis. We need, therefore, not only to bring the head over the socket, but at the same time to raise up the trochanter and neck, so as to allow the head to enter. Now, in the ordinary methods of reduction, this raising up of the trochanter, so as to put the neck in the proper direction for the head to enter its socket, is done first, by the action of the pulleys, and the approximation of the head to the socket is done second, by the continuation of the extension. This raising of the trochanter is, of course, opposed strongly by the muscles inserted into its base, causing the head to be pressed more and more firmly against the pelvis, and increasing the friction, and thereby causing, by far, the greater part of the difficulty in bringing down the head to the level of the acetabulum. It is in this, principally, and I am myself disposed to say only, that any active muscular contraction opposes the reduction of a dislocation of the hip-joint. True, the large muscles around the joint are thrown into action as soon as extension is made; but this is an action excited by the extension, and, that it is a very feeble opposing force, is evidenced by the facility with which these muscles give way to the force of a single unaided arm, when a fracture of the neck of the femur is concerned, in which, of course, none of the friction alluded to can occur. This comparative action of the muscles, in fracture and in dislocation, is very strongly and appropriately insisted upon by Dr. Reid.

"The process by manipulation avoids this main difficulty, and, as it were, eludes the opposition of the muscles. The trochanter, being fixed by the insertion into its base of the pyriformis, the two obturators, the gemelli, and the upper part of the quadratus, acts as a fixed point or fulcrum, upon which, by moving the limb, the head of the bone can be made to describe a circle round the fulcrum. When we remember that this fulcrum is not, strictly speaking, a fixed point, but has a certain degree of motion of its own, we can easily see how, by means of this movable fulcrum, the head of the bone can be placed, by varying the motions of the limb, on almost any point within two inches around the acetabulum, and, of course, over the acetabulum itself. If this manipulation is made in such a way as not to raise the trochanter from lying against the pelvis, then, when the head comes over the acetabulum, a slight rotation, such as is given by the rocking motion employed, will sufficiently raise the trochanter to let the head slip in without provoking to opposition the trochanteric muscles, and if the movements be made in such a direction as to relax the stretched muscles, the whole may be accomplished without calling forth the slightest muscular opposition from the beginning to the end of the procedure. This principle, in its application to the different forms of dislocation, presents some variations. In the dislocations on the dorsum, and on the ischiatic notch, for their mechanism is for our purpose identical, the principle has its best illustration; and, if any one will take the skeleton or the dead subject, and go through the process, he will perceive that, by adduction, the tense rotators are relaxed, and that, by flexion of the thigh upon the trunk, the head is caused to pass down behind and below the acetabulum, and then, by carrying the knee out so as to abduct the limb, that the head comes toward the lower portion of the acetabulum where its margin is least prominent. At this point, I wish it to be observed, that our mode of procedure varies a little from Dr. Reid's. He recommends, when the head is brought by abduction close to the lower edge of the acetabulum, that, by the rocking movement already described, it be caused to slip in. This is well, and will probably answer in many cases, but it failed us so completely from the first, that we were led to add the bringing down of the thigh to the straight position in a state of abduc-

tion, still keeping up the rocking motion, and it has been uniformly in the act of thus bringing down the limb, that the reduction has been accomplished. On looking at the parts in the dead subject, it will be seen that this movement of the limb, when the head has reached the lower margin of the acetabulum, tends directly to roll the head upwards over the edge and into the socket. The mechanism of the reduction from the foramen ovale has already been alluded to. I do not know of any case of reduction from the pubes.

"If the proposed plan should prove, on further trial, to be as successful and as free from danger as it thus far has been, one most valuable feature of it, as a surgical resource, will be its availability. Wherever a surgeon, with his bottle of ether, can go, there the dislocated hip can be reduced, without instruments, without appliances, without assistants."

"Everything," Dr. Markoe adds, "in our experience thus far seems to indicate that this method of reducing luxations is as safe, if not safer, so far as the integrity of the joint and its after usefulness is concerned, than the reduction by forcible extension with the pulleys. The method is not, however, without its dangers, and these mainly arise from the immense amount of force which can be exercised by acting on the long arm of so powerful a lever as the whole limb, while the short arm has at most a length of three inches. By the inconsiderate or misdirected action of this lever force, one of three accidents might be produced: either the muscles holding the trochanter, and thereby forming the fulcrum on which the power is applied, might be torn from their attachments; or, if they held fast, the tissues, round and among which the head passes in its movements, may be extensively lacerated or contused; or, lastly, it seems to me very possible that the neck of the bone might be broken by too violent abduction, forcing it against the side of the pelvis. Though happily these dangers are thus far only theoretical, yet the anatomy of the part shows that they are real, and each one of them might have most serious consequences. It would not be possible to lay down any specific rules whereby these dangers are to be avoided, except by insisting on that almost universal law of surgical manipulation, that everything is to be done with gentle moderate force, and never with sudden violence. As far as my recollection serves me, and I myself assisted in almost every case reported, we never have accomplished anything by proceeding in a direction where great force was required to continue the movement, but have always succeeded by finding a direction in which the mere continuance of the movement, without force, has brought the head into the proper position. It will be noticed, by looking over the cases, that in many of them, before the head went into its socket, it slipped about on the outer surface of the pelvis, taking sometimes one and sometimes another of the four positions usually spoken of as the four forms of luxation of the hip. This in some instances happened several times, and in the two instances of failure, this change of position was all that could be accomplished. Now, it cannot be doubted that this extensive movement of the head of the bone must be attended with a corresponding amount of laceration and displacement of the tissues among which it passes, and although the mischief thus done may be confined to the areolar tissue in the muscular interspaces, still it is an injury which may add to the dangers of after-inflammation, and is, if possible, to be avoided. I suppose this can only be done by defining more accurately the precise method of procedure to be adopted in each case, so that no experimental or ineffectual trials shall have to be made, but the operator shall be able at once to do exactly what is necessary to bring the head of the bone to the point desired."

The following case, given at the conclusion of Dr. M.'s paper, has such important bearing upon some of Dr. M.'s remarks that we quote it:—

"Patrick Barry, aged 42, was admitted to the New York Hospital, Oct. 23, 1854, with a dislocation of the left femur, which had occurred seven weeks previously, by a fall from a rail-car while it was in motion. The symptoms were unequivocal, the limb being shortened 1¾ inches, the ball of the great toe resting on the instep of the sound foot, and the head of the bone being distinctly felt upon the dorsum of the ilium. The patient was a man of good muscular development, but the injured limb was somewhat wasted and flabby from inaction. Two days after admission he was put under the influence of

ether, and Reid's manipulation was tried. The head descended as usual, until it came opposite to the lower margin of the acetabulum, but from that point, as the limb was brought down, it slipped on to the foramen ovale. The manipulation was repeated several times, with all care, varying the degree of abduction on the various trials, but without success. It was impossible to make the head rise over the lower border of the acetabulum so as to slip into its place. After numerous thorough and careful trials, the manipulation was abandoned and the pulleys ordered to be applied. Before this was done, it was thought best to place the head of the bone on the foramen ovale, and from that point to try and reduce it by the usual method recommended by Sir Astley Cooper. The head was accordingly placed on the foramen, and while the upper part of the thigh was grasped by an assistant and lifted strongly outwards, I took hold of the ankle and made extension and adduction. The head seemed not to move at all under this force, and while making strong adduction a crack was heard, everything became loose about the joint, and on examination it was evident that a fracture of the cervix had taken place, leaving the head in the foramen ovale. There was nothing further to be done but to put the limb up in the straight apparatus, hoping that, if we could obtain union, he would have as useful a limb as those ordinarily left by fracture of the cervix, and certainly a better limb than if the dislocation had been untouched. Thus far, Nov. 25, everything has gone well, and promises union, with a shortening of about an inch. I am sorry that we must accept this case as one of failure of the new plan after what we considered a fair trial; for myself, however, I do most profoundly believe that it failed simply because we have not yet learned enough about the manipulation to adapt it to the condition of parts concerned in this particular instance. That we shall yet acquire that knowledge, I see no reasonable ground to doubt. With regard to the fracture of the cervix, we were all surprised at the slight amount of force which was competent to produce such a mortifying accident. It adds double force to the caution given above, when speaking of the possibility of that accident, and it is not a little remarkable that the paragraph containing that caution was written on the very morning of the day when the production of the fracture verified the necessity of the warning. Dr. Watson, in a note to me, speaks of a fact which, he says, 'I have on undoubted authority, viz: from one of the professors in the School of Medicine in Toronto, Ca., that an accident, similar to that of Patrick Barry, occurred in that city, while the surgeon was attempting to reduce a luxation of the hip by Reid's method.' Finally, it must be observed that the new plan is entitled to none of the blame of the fractured cervix. The accident took place after Reid's manipulation was abandoned, and while we were attempting the reduction according to the old established and classical method."

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Exsection of the Head of the Femur, and Removal of the Upper Rim of the Acetabulum, for Morbus Coxarius.—Dr. Lewis A. Sayre records (*New York Journ. Med.*, Jan., 1855) an interesting case of this. The subject of it was a girl, nine years of age, who had been suffering for 18 months with the disease, and which resisted the usual treatment. When seen by Dr. S., an abscess had formed, involving the whole upper front and inner portion of the thigh, accompanied with chills, profuse sweats, and great prostration. The abscess had pointed in two places, and was apparently just ready to open.

"The leg was shortened 2¼ inches, and turned inward, *but not permanently fixed in its position* (as is usual), but allowing of considerable motion, which gave a distinct *bony crepitus* between the femur and ileum. The pelvis was twisted and drawn upwards. Her general health had become much affected, having lost her appetite, and she was suffering from hectic, with constant chills and profuse sweats, and was only rendered comfortable by the constant use of anodynes."

Dr. S. advised a free opening of the abscess, and, if necessary, the removal of the head of the femur. This was at first objected to by the father, but subsequently consented to, and the operation was performed on the 29th March, 1854.

Dr. S. first laid open the abscess by a free incision of about six inches, over the trochanter major, on the outer aspect of the thigh, and in a line with the femur, and then cut into the floor of the abscess (which principally occupied the inner and front portion of the thigh), and discharged about a pint of thin serous and flaky pus. The finger was then readily passed around the neck of the femur, and detected an opening in the capsular ligament on the inner surface of the neck. The upper border of the acetabulum had been absorbed, and the head of the femur was upon the dorsum of the ileum, near the anterior superior spinous process, *surrounded by its capsule* (which seemed to have been slipped up), and a large deposit of bone, apparently being an attempt of Nature to make a new acetabulum. But this cavity thus formed had no lining membrane, as the femur grated roughly upon it. Dr. S. then opened the capsular ligament on a line with the external incision, and disarticulated by bringing the leg strongly across the opposite thigh, and then, with a large pair of Luer's forceps, readily cut off the head of the femur, at the lower extremity of the neck. The bone at this point appeared perfectly healthy. Dr. S. was very cautious not to injure the insertion of the psoas magnus, or iliacus internus, or any of the rotator muscles, which are inserted just behind the trochanter major.

The upper rim of the acetabulum had been absorbed (according to the theory of Dr. March, of Albany), and the new deposit of bone, which was intended to supply its place, was denuded and carious. Dr. S. gouged it off with a sharp, firm chisel, made for that purpose, and, in this way, took off a number of flakes of bone, until he came to a healthy, bleeding surface.

The anterior superior spinous process on its outer surface, and the external lip of the crest of the ileum, was black and carious for some distance, and with the forceps he easily clipped it off until he came to healthy bone. Very little blood was lost in the operation, and after cleaning away all the debris, he brought the leg in the straight position, filled the wound with lint, and dressed with a roller and cold water compress. She was then put to bed, and a cup of strong coffee administered, after which she soon fell asleep.

The child was under the influence of chloroform during the operation, which occupied nearly 20 minutes, and was perfectly insensible the whole time.

The case went on favourably, and, on the fifth of April, Dr. S. applied the straight splint, for counter extension, to the well side, and made extension by means of the foot-board, bringing the limb down to the same length of the opposite. Though some unfavourable symptoms subsequently presented themselves, the cure progressed. After the 1st of August, at each dressing, the body was brought to a right angle with the thighs, to prevent anchylosis.

On the 1st September, when the bandage was removed, there seemed no tendency to retraction of the limb. The splint was reapplied, but the body was left free from the bandage, so as to allow of flexion, in order to prevent anchylosis. By the middle of October, began to walk on crutches, and on the 20th November, Dr. S. reports:

"To-day I placed her in the horizontal position, and measured her carefully, and find there is $\frac{3}{8}$, or nearly $\frac{1}{4}$ of an inch shortening. By taking hold of the foot, the whole body can be drawn down in bed without pain in the joint, and a pressure may be made sufficiently strong to move the pelvis and body upward without producing any shortening of the limb. When she lies upon the back, with the leg extended upon the thigh, she can elevate the heel sixteen inches from the bed, and flex the knee so as to bring the thigh at a right angle with the pelvis; she can rotate it internally so as to touch the other foot, and externally so as to touch the bed. Her general health is perfect, and the case has terminated perfectly successfully."

Dr. S. gives a table of thirty cases, in which excision of the head of the femur has been performed for morbus coxarius; of these, 20 recovered, and 10 died. Of the former, 13 were completely successful; 3 died of an intercurrent disease, at periods varying from three months to two years after the operation; 1 is reported as not having progressed favourably; the remainder were too meagrely reported, or too recently performed to decide correctly of results.

Of the latter, 4 died within one week after operation; 1 on the 12th day; 2

in two months; 1 in four and a half months; 1 in some months after; 1 unsuccessful.

Perineal Section for Stricture of the Urethra.—The *New York Journ. of Med.* for March, 1855, contains an interesting article by Dr. Fred. D. Lente, on this operation, which our readers are aware has been latterly revived by Mr. Syme, of Edinburgh, and its propriety has given rise to much angry discussion in Great Britain. Mr. Syme never performs the operation without first introducing a guide into the bladder, which he insists can always be done, there being, he maintains, no such thing as an *impermeable* stricture. Mr. Fergusson has lately performed this operation in several cases in King's College Hospital, and in his clinical lecture on the occasion, asks whether the operation can be performed in cases in which no instrument can pass. Mr. Lente expresses his surprise at this query, and states that the perineal section, both with and without a guide passed through the stricture, has been by no means an uncommon operation in New York city, for years past, and refers to a table embracing twenty-seven cases of this operation, published by him in the 4th Vol. of the *Transactions of the American Medical Association.*

"The operation of external division without a guide is looked upon," Dr. L. states, "in the New York Hospital, as so practicable and so successful, though certainly, at times, one of the most annoying and difficult of operations, that, in the cases of obstinate retention of urine admitted into the institution during our connection with it, where the introduction of any instrument was found to be absolutely impracticable, the practice of puncturing the bladder was never once resorted to, but relief was always attempted by the perineal incision. In the London hospitals, on the contrary, it would seem that the operation of puncture is generally preferred. Mr. Hilton, Surgeon to Guy's Hospital, says that twenty or thirty such cases have occurred there during the past few years, and reports three cases himself in one number of the *Lancet;* in one of which, the catheter having slipped from the opening (into the rectum), the operation was necessarily repeated *de novo* on the succeeding day; all of these three operations were successful. The operative procedure, in cases where the obstruction is impassable, is briefly this: The patient having been secured in the usual position, a medium-sized catheter or sound is passed down to the stricture and kept in position by an assistant; the incision is then carried down in the median line to the end of the instrument, which is thus exposed; it is then continued downwards towards the posterior end of the stricture (which is usually of some length), and the anterior point of that part of the membranous portion, still unobstructed, and generally *distended* with urine, diligently sought for. This constitutes the only difficult part of the operation, and may occupy from five minutes to an hour or more; generally, from five to fifteen minutes, however, have sufficed to discover it. As soon as a fluctuating point is perceived, either by the touch or by an experimental incision in the proper direction, an opening is made with a sharp-pointed bistoury, a director introduced, and then the end of the instrument, previously introduced through the distal portion of the urethra. It has been the usual custom to retain a *silver* instrument in the canal, secured by an appropriate bandage, as being the most cleanly. This is removed from time to time, and cleaned. In some cases, when the knife or director has been inadvertently removed from the posterior part of the divided urethra, after all the urine has been evacuated, and before the catheter has been passed into the bladder, it has been extremely difficult to find the opening again, and thus to complete the last step of the operation; especially in cases where the anatomical relations of the parts have been disturbed by the existence of abscesses and fistulous canals.

"Regarding the question, so much mooted on the other side of the Atlantic, as to the possibility of passing an instrument in *every* case of stricture, we have only to say for ourselves, on this side, that, in many cases, our best surgeons, after the most persevering and gentle trials with all kinds of instruments, are sometimes completely foiled, and these are the cases to which the operation under discussion has been applied. Most, if not all of the cases contained in the Table were of this character, and all of those which are mentioned as hav-

ing occurred in private practice, with, perhaps, one exception, this having been complicated by large abscesses, and by fistulas. We are convinced of the fact which Mr. Fergusson says he has long had the idea of—'that the seeming dexterity (in passing instruments in these impermeable strictures) was nothing but actual violence;' and that surgeons 'appear to pass the instrument easily into the bladder, whilst they are *really forcing* their way through a canal, the relative anatomy of which is very familiar to them.' We may, in this connection, remark that, in the practice of the New York Hospital, when even the smallest-sized conical instrument can be introduced, the idea of dividing the perineum, as a means of cure, is not entertained, but a cure by gradual dilatation, with graduated instruments, is confidently expected; we remember no instance of a different result. This being the case, the first step in the operation, as performed by the great Scotch surgeon and by the surgeons of the London hospitals generally, is an argument against the propriety or necessity of any cutting operation whatever; for, why should a patient be subjected to so severe a procedure as the perineal section, when the passage of the guide through the stricture may be followed by larger instruments, and the canal thus enlarged to its normal dimensions? In cases complicated by abscess or by extensive fistulas, it may be, and is often, necessary to perform the cutting operation, even though a small instrument can be introduced. In a case which we have recently had under treatment, and which we may be permitted to introduce here as an exemplification, it was only after repeated trials, even with the important aid of an anæsthetic, and with considerable difficulty, that we could introduce an instrument, and then only one of the fine-pointed, flexible bougies manufactured by the French, and so admirably adapted to these cases, its diameter, at its largest part, being only one-half that of silver catheter No. 1, and yet, in the course of three or four weeks, by careful and gradual dilatation, the urethra was enlarged to the size of No. 10."

Of the twenty-seven cases contained in Dr. Lente's table, three died, two were much relieved, four were relieved, and eighteen were cured. "In some of the cases which were discharged as relieved, a further operation might have resulted in cure, but the patients were unwilling to submit to it," being contented with the alleviation of their sufferings already afforded. In many instances in which the operation was performed, it was under very unfavourable circumstances. In seventeen cases, in which the duration of the existence of the stricture, previous to the operation, is mentioned, the average is seven years. "In most, if not all of the cases, the operation was regarded as the only means of effecting a cure, all other means having failed; and, in many instances, it was necessary, not only for the purpose of rendering the patient's life more comfortable, but for saving it. The proportion of cutting operations for stricture, to the number treated by dilatation, is very small in the New York Hospital."

"We look upon it as a matter of the greatest importance, to establish the feasibility and comparative safety of the perineal section, and especially as performed without a guide, for in most, if not all cases requiriug the operation, surgeons find it impracticable to introduce any guide, although one of small size may, perhaps, usually be forced through by one well acquainted with the course of the canal and its relations, but in no case, we think, without danger of making false passages, and thus materially increasing the danger, and diminishing the chances of a perfect cure from the subsequent operation. The cases included in our Table, and those performed in private practice in the city, with like results, will, in our opinion, go far towards increasing the confidence of surgeons in this operation."

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Ovariotomy.—This operation was recently performed by Dr. A. Mercier, of New Orleans. The patient was a woman, 28 years of age, the mother of four children. A swelling was first observed in her right iliac region, about a year before. Six months afterwards she was tapped, and six gallons of a white, thick albuminous fluid drawn off. The effusion rapidly accumulated again, and she was tapped six times subsequently. The operation was performed on the 17th Dec. 1854. The patient having been put under the influence of chlo-

roform, "an incision, nine inches in length, was made over the tumour, extending from the lower ribs to the external edge of the rectus abdominalis. Two small muscular arteries were tied during the course of the incision, and but little blood was lost, either then or subsequently. With some difficulty the adhesions which bound the tumour to the abdominal parietes, the liver, the iliac fossa, and to the omentum, were torn apart with the hand, the intestines, &c. being freely handled for this purpose. Near the pedicle of the tumour the adhesions were so strong, for more than an inch in extent, that the bistoury had to be resorted to. The tumour presented the appearance externally of an enlarged ovary, and such was its size that, in order to draw it out of the abdominal cavity, several deep incisions had first to be made into it, and a large portion of its fluid contents to be evacuated. Strong, thick twine was then tied as forcibly as possible around the pedicle of the tumour, and it was excised about half an inch above the ligature. The internal parts were freely sponged, and the lips of the wound brought together with five sutures, which included all the parts incised—the peritoneum, muscles and integument. The lips of the wound were further approximated with adhesive straps, and covered simply with lint. Openings were left, sufficiently large to admit a ready egress to any fluid contents of the abdomen.

"The tumour, when extracted, measured nine inches in length, and six in diameter. Its weight could not have been less than six pounds. It was of a fibro-cartilaginous character, and composed of a mass of small conglomerated cysts of various sizes, containing a fluid of the colour and consistency of glycerin.

"By the seventh day, the muscular portions of the wound had united, except opposite to the sutures. On the thirteenth day, the ligature came away, and indicated by its appearance that, with a little force, it might have been removed without danger several days before. From this period the flow of serum through the wound, which at first had been considerable, has rapidly decreased."

At the date of the last report, seventeen days after the operation, the wound had almost entirely healed, and the patient was sitting up.—*New Orleans Med. and Surg. Journ.*, Jan. 1855.

The entire Scrotum and Perineum, with one Testicle and its Cord attached, and nearly all the Integuments of the Penis torn off.—Dr. Robert W. Gibbes, of Columbia, S. C., relates (*Charleston Medical Journal*, March. 1855) a very interesting case of this. The subject was a coloured man, 22 years of age, who was caught by the shafting in a foundry, and the following sustained: "The entire scrotum was torn off, with the skin and cellular tissue, from two and a half inches above the spine of the pubis down to the edge of the sphincter, and including all the breadth of the perineum. together with the left testicle with five inches of its cord attached, and all the integument and cellular covering of the penis, except a rim nearly half an inch wide at the extremity, and continuous with the mucous membrane of the prepuce.

When seen by Dr. G. the patient was suffering most acutely. Chloroform was immediately administered, and on examination the right testicle was seen hanging down by its bare cord, and apparently covered only by the tunica vaginalis communis as high as the abdominal ring, where the elastic feel of the intestines was distinctly perceptible, as well as on the opposite side, where the end of the cord was so retracted as to render its ligation impossible. Not more than half oz. blood could have been lost, as scarcely any oozing was observed, and the parts separated were almost as clean as if they had been sponged. The gap in the perineum was brought together by three points of the interrupted suture, and, as it was impossible to form a covering for the raw surface and remaining testicle, a simple lint dressing was applied, the penis supported on the left thigh, and the testicle on a small bran cushion placed between the legs.

The patient recovered, and his sexual appetite, he says, is as great as before the accident, and coition as agreeable as ever; the only difference, he observes, is that the semen "does not come as free." One of his fellow-workmen said

to me, "Oh, he is as good a man as ever;" to which Henry replied *that he believed he was.*

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Treatment of Ulcers by Anaplasty.—Having in our preceding No. (pp. 276–78) noticed the rival claims of Dr. F. H. HAMILTON and Dr. JOHN WATSON, to a method of treatment of ulcers by Anaplasty, it is but justice to the former surgeon to state that in a subsequent reclamation (*New York Journ. Med.*, Jan. 1855) he denies that Dr. Watson has not anticipated him in either the proposal or the results; but states that Dr. Watson has misapprehended the principle of his operation. This operation consists in transplanting a piece of new and perfectly healthy skin upon the *centre* of the ulcer, by which means he hopes not only to supply an amount of skin equal to the size of the piece transferred, but to furnish, also, a nucleus from which additional skin shall be formed—to establish a new centre of life—an oasis, from whose outer verge a true and healthy vegetation shall advance in every direction over the exhausted soil.

"It is not improbable," he remarks, "also, that the graft will itself expand, or be drawn centrifugally by the contraction of the surrounding granulations and cicatrix, conversely, as the skin about the ulcer had before been stretched and drawn centripetally, by a similar action of the granulations and cicatrix situated within its free margin, so that, after a time, it will cover more space, independent of any actual growth, than it did originally. The opposite of this happens usually in anaplasty, and would occur here, did the flap equal or exceed in size the wants of the parts to be supplied. The flap would contract, thicken, and project itself above the surface. But in old ulcers, it will generally be found impossible to furnish a direct supply of integument equal to the loss. A deficiency must probably still exist, and sufficient, it is believed, to determine in the transplanted skin a necessity of expansion."

He further states that his proposal dates from the year 1844.

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Fungus Hæmatodes of the Eye; Extirpation; Recovery.—Dr. JAMES BOLTON relates (*Stethoscope*, Feb. and March, 1854) a case which he considers to be an example of fungus hæmatodes of the eye, occurring in a negro, 35 years of age. The disease was not traceable to any injury; its progress was slow, and for a long time painless. The system had begun to sympathize deeply, exhibiting a cachectic condition. The eyeball was very much protruded. Dr. Bolton extirpated the whole globe. A section of it afterwards showed a fungous tumour of a brownish colour occupying almost the whole interior of the ball, appearing to spring from the ciliary body, and including the iris and choroid coat. The lens and vitreous humour had disappeared; a small quantity of fluid, containing reddish granular particles, was evacuated; the optic nerve was sound. The patient continued well twenty months after the operation.

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Results of Twenty-four Operations for Lithotomy.—In the No. of this Journal for July, 1850, Dr. P. C. SPENCER, of Petersburg, Va., reported the results of fifteen operations for lithotomy, and in the *Virginia Medical and Surgical Journal*, for February last, he reports nine more cases. Of these nine, 6 were whites, 5 boys, and 1 man 64 years of age; three were blacks, 2 boys, and 1 man aged 61.

Of the twenty-four cases, Dr. S. lost but two, a proportion of 1 in 12.

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Choleraic Epidemic at the Massachusetts State Prison in July, 1854.—Dr. WM. B. MORRIS, Physician to the Massachusetts State Prison, gives (*Boston Medical and Surgical Journal*, March 15, 1855) the following account of the outbreak of cholera in that institution in July last:—

"At midnight, July 27, I was called to attend Peter York, a coloured convict, who had been in solitary confinement for nearly seven years. On arriving at the prison, I found him with a feeble pulse, cool skin, cramps of the abdomen and extremities, purging and vomiting. Thinking it an ordinary case of cholera morbus, I prescribed the usual remedies, and as soon as relief was afforded, returned home. I had scarcely gotten into bed when I was again called; and before reaching the prison a second time, four new cases had occurred

in different parts of the institution. The attacks became from this time so frequent that I was obliged to remain constantly in the hospital, which soon became crowded with patients. During the whole of the 28th, the succeeding night and following day, new cases continued to occur, so that in forty-eight hours from the commencement of the epidemic 205 convicts had been more or less severely attacked. The symptoms varied in intensity in different cases, but in all were characterized, to a greater or less degree, by lividity of the lips and surface of the body, cold extremities, painful cramps, vomiting and purging. In a few instances, also, there was suppression of urine.

"The *causes* of this sudden outbreak are, to my mind, wholly conjectural. The report that it depended upon diseased beef is entirely without foundation. *No beef had been given at all.* The ration on the 27th, consisted of 'dun-fish' (Mr. Mason and myself examined the lot from which they were taken, and found them to be of the best quality), rice and bread. On the next day there was an entire change of food. I have forgotten now what the diet for that day was, though I have it somewhere among my papers. Yet notwithstanding this change, 86 new cases occurred. There was a very sudden change of temperature on the evening of the 27th, from excessively hot to uncomfortably cool weather. I suffered so much in going to the prison to visit York, that I borrowed an overcoat to wear home. This change of temperature, together with the fact that cholera was prevailing at the time, has always inclined me to believe that atmospheric causes played an important part in the production of this sickness."

—

Employment of Injections into the Bronchial Tubes, and into Tubercular Cavities of the Lungs.—Dr. Horace Green, of New York, claims (*American Medical Monthly*, Jan. 1855) to have treated with advantage bronchial affections and tuberculosis, "by the direct introduction into the lungs of a strong solution of nitrate of silver injected through" an "elastic tube." The subject was brought before the New York Academy of Medicine on the 6th of December last, and referred to a committee to inquire into and investigate the proposed treatment (*New York Med. Times*, Jan. 1855). Although nearly four months have now elapsed, this committee have not yet made a report. It will be time enough when the projector shall have satisfied the committee of the practicability of his procedure, and of its utility, if practicable, to give it further notice.

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Vesico-Vaginal Fistula cured by Sims's Operation.—Dr. A. V. Williams records (*New York Med. Times*, March, 1855) a case of vesico-vaginal fistula, the opening large enough to admit two fingers, cured by Dr. Sims's operation.

AMERICAN MEDICAL ASSOCIATION.

The eighth annual meeting of the American Medical Association will be held in the city of *Philadelphia* on Tuesday, May 1, 1855.

The secretaries of all societies, and other bodies entitled to representation in the Association, are requested to forward to Francis West, M. D., 352 Chestnut St., Philadelphia, one of the secretaries, correct lists of their respective delegations *as soon as they may be appointed;* and it is *earnestly* desired by the Committeee of Arrangements that the appointments be made at as early a period as possible.

MEDICAL SOCIETY OF THE STATE OF PENNSYLVANIA.

The annual session of the Society will be held in Hollidaysburg, on the last Wednesday (30th) of May, at 10 o'clock in the forenoon.

The secretaries of the several County Societies will please forward the lists of delegates to either of the Secretaries, D. Francis Condie, Philadelphia, or Henry Carpenter, Lancaster.

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